THE

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Notices of the Aëronautical Society of Great Britain

GENERAL NOTICES

February Elections: *Members:*—C. PERCIVAL CLARK; LIEUT. C. J. L'ESTRANGE MALONE, R.N.; C. SPENCER PAYNE.

Student:—CARLYLE FLIEDNER.

March Elections: *Members:*—C. GRAHAME-WHITE; BRIG.-GENERAL D. HENDERSON, D.S.O.

April Elections: *Members:*—SIR GEORGE GREENHILL, F.R.S.; J. L. HODGSON, B.Sc.; H. D. DE M. CAREY; ENG.-LIEUT. CHAS. RANDALL, R.N.; G. P. OTTINO.

Student:—J. C. SAVAGE.

¶ **Vice-Presidents.** The Most Hon. THE MARQUIS OF TULLIBARDINE, M.V.O., D.S.O., M.P., and E. P. FROST, Esq., D.L., J.P. (Past President of the Society), have accepted the invitation of the Council to become Vice-Presidents of the Aëronautical Society.

¶ **Election of Chairman.** MAJOR-GENERAL R. M. RUCK has been elected Chairman of the Council for 1912–13.

¶ **Council Election.** As the result of the recent election, the Council for 1912–13 is composed as follows:—*Associate Fellows:* GRIFFITH BREWER; CAPT. A. D. CARDEN, R.E.; T. W. K. CLARKE; J. W. DUNNE; J. H. LEDEBOER; ALEX. OGILVIE; MERVYN O'GORMAN; F. HANDLEY PAGE. *Members:* A. E. BERRIMAN; COL. J. E. CAPPER, C.B., R.E.; B. G. COOPER; LIEUT. R. GREGORY, R.N.; CAPT. E. M. MAITLAND; F. K. MCCLEAN; COL. H. E. RAWSON, C.B., R.E.; MAJOR-GENERAL R. M. RUCK.

¶ **Associate Fellow Election.** The next election of Associate Fellows will be held on June next. The last day for the receipt of applications is TUESDAY, MAY 28, and the result of the election will be declared on Wednesday, June 12.

Application forms may now be obtained from the Secretary.

¶ **Appointment of Committees.** The following Committees has been appointed :—

(1) *Inventions Committee* (which deals with inventions submitted for the Society's opinion) : T. W. K. Clarke, B. G. Cooper.

(2) *Library Committee* (which has charge of the Society's library and records) : H. F. Lloyd, Harry Turrill.

(3) *Publications Committee* (which controls and organises the Society's publications) : A. E. Berriman, B. G. Cooper, J. H. Ledeboer.

(4) *Relations Committee* (which treats, when occasion arises, with the aeronautical bodies) : A. E. Berriman, B. G. Cooper, F. Handley Page, Major-General R. M. Ruck.

(5) *Research Committee* (which conducts the research work of the Society in all its branches) : Harris Booth, T. W. K. Clarke, B. G. Cooper, Col. J. D. Fullerton, B. Melvill Jones, Archibald R. Low, Mervyn O'Gorman, F. Handley Page, A. P. Thurston.

¶ **Representatives.** The following have been appointed to represent the Society on the indicated Committees :—

(1) *The Royal Aero Club's Committee for the investigation of Aeroplane Accidents :* A. E. Berriman, J. H. Ledeboer, Mervyn O'Gorman.

(2) *The Parliamentary Committee on Aeronautics :* Lord Montagu of Beaulieu, Griffith Brewer, J. W. Dunne.

(3) *H.M. War Office Technical Reserve Committee :* Col. J. D. Fullerton, R.E.

¶ **Assistant Secretary :** Mr. Harry Turrill has been appointed Assistant Secretary from March 1, 1912.

¶ **Pilcher Memorial Fund.** This fund is now closed, and a cheque for £22 12s. has been forwarded to Lord Braye. The work of erecting the memorial column at Stanford Park has been commenced, and it is hoped that the unveiling will take place in June.

The Council begs to thank the donors for the following subscriptions :—

	£	s.	d.
Amount previously acknowledged	14	13	0
Mrs. Tidswell	2	0	0
C. H. M. A. Alderson, Esq.	1	1	0
Major B. Baden-Powell	1	1	0
Griffith Brewer, Esq.	1	1	0
Alex. Ogilvie, Esq.	1	1	0
Col. H. E. Rawson, C.B., R.E.	0	10	0
Col. F. G. Stone, R.A.	0	10	0
W. H. A.	0	5	0
A. E. Berriman, Esq.	0	5	0
C. C. Turner, Esq.	0	5	0
	£22	12	0

¶ **The late Prof. Rotch.** Upon the receipt of the news of the death of Prof. A. L. Rotch (Director and Founder of the Blue Hill Observatory, U.S.A.), which occurred at Boston following appendicitis on April 7 last, the Council passed the following resolution, which has been communicated to the Observatory :—

"The Council of the Aëronautical Society of Great Britain deeply deplores the great loss to Meteorology and Aëronautics of Professor Abbott Laurence Rotch, whose devoted and unsparing labours have contributed so much to Science, and desires to express its sincere sympathy with the Blue Hill Meteorological Observatory."

¶ **Committee of Enquiry.** The Committee of Enquiry, which was appointed on March 31, 1911, to report and advise on the Society's position, has now handed over to the Society a balance of £12 13s. 6d., which has been placed to the credit of the Society's General Fund. When entering upon its duties, the Committee appealed for donations from the members in order that its work might be carried out without expense to the Society as a whole.

As a result of this appeal, the following sums were received, which the Committee begs to acknowledge :—H. Massac Buist, £10 10s. ; A. E. Berriman, Griffith Brewer, E. A. Davies, J. W. Dunne, W. T. Douglass, J. MacHaffie, Alex. Ogilvie, M. O'Gorman, F. Handley Page, each £1 1s. ; Hon. H. S. Littleton, J. F. Ogilvey, Leslie Stephens, R.A., Col. F. G. Stone, R.A., each £1 ; C. H. M. Alderson, 10s. ; T. W. K. Clarke, 7s. 6d. ; S. F. Edge, C. G. Grey, Gen. Sir Richard Harrison, K.C.B., H. F. Jackson, H. F. Lloyd, Alex. McCallum, Andrew Stewart, C. C. Turner, C. E. I. Wright, E. W. Young, each 5s. ; S. Cowper Coles, P. Narraway, Miss D. Prentice, A. P. Thurston, each 2s. 6d. Total, £27 16s. 6d.

The expenses incurred, including printing, typing, and postage, amounted to £15 3s., leaving a balance as aforesaid.

¶ **Gifts to the Library.** The Council begs to thank Lieut.-Colonel O. Smeaton for a copy of his lecture on "Aerial Scouting," and Messrs. A. V. Roe and Co. for the gift of lantern slides of their recent machines.

OBITUARY

DOUGLAS GRAHAM GILMOUR

It is our painful duty to chronicle the death of Mr. D. G. Gilmour, who was killed by a fall with a Martin-Handasyde monoplane on February 17 last in the Old Deer Park, Richmond. The cause of the accident was, and still remains, a mystery. Eye-witnesses stated that the wings broke and folded back, but examination of the debris revealed the astonishing fact that all the wing wires were intact as well as the warp wires. It is conceivable that Mr. Gilmour was taken ill while flying, and that the witnesses—none of whom, unfortunately, had technical knowledge of aeroplanes—were deceived into thinking that the machine broke.

Mr. Gilmour was born in 1885, and took his certificate on a Blériot at Pau in April, 1910. He was one of the most skilful and daring of all English flyers—daring, but not reckless, and taking no unnecessary risks. Among his best-remembered feats were his flight on July 5, 1911, up the Thames to London Bridge and back to Brooklands, his following the 1911 Boat Race from start to finish, and his appearance at the Henley Regatta of the same year. The burial took place at Mickleham amid a large concourse of mourners, the Society being represented by the Secretary.

ANNUAL GENERAL MEETING

THE Annual General Meeting of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, on Wednesday, 27th March, 1912, at 8.30 p.m.

The Chairman of Council, Mr. A. E. Berriman, presided, and opened the proceedings by reading the Notice convening the Meeting.

At the call of the Chairman, Messrs. A. Arkell-Hardwick, H. F. Lloyd, and G. P. Deverall Saul volunteered their services as scrutineers of the Council Election, and left the meeting with the ballot box in their custody.

Council's Report and Balance Sheet was presented to the Meeting.

COUNCIL'S REPORT 1911-1912

THE present Council assumed office with the responsibility of carrying out the new rules that were adopted at the General Meeting of September 25, 1911.

These rules provided for :—

1. The formation of a company to limit the liability of members (Rule 55) ;
2. The creation of a nucleus of a technical side within the Society, by the election of Associate Fellows (Rule 5).

The Council is pleased to be able to report on these items as follows :—

Aerial Science Limited.—Through the unremitting labours of the Society's Honorary Solicitor, the tedious and by no means easy task of giving effect to Rule 55 has been most satisfactorily concluded by the formation of Aerial Science Limited. This is a private limited company, the Directors of which are the Members of the Council of the Society for the time being. It limits the liability of Members without influencing the Society's government and without profiting any Member of the Board.

The Council believes that the arrangement is perhaps the best yet devised to overcome the many difficulties of a problem that is common to all similar institutions, and considers that the Society is very deeply indebted to its Honorary Solicitor for his invaluable help.

Associate Fellows.—A form of application having been sanctioned, a notice advising members of its readiness and fixing the latest date for nominations was duly circulated.

Forty-seven applications were received and each separately was most carefully considered by a thoroughly representative Council.

Under the rules the Council was required to decide whether a candidate "had attained to an acknowledged position in the science of aeronautics" (Rule 4b), having regard to the clauses contained in Rule 17.

As the result of these investigations the Council recommended thirty-five candidates for election. In several cases the Council was unable to recommend candidates who may have been eligible, because their application forms contained insufficient particulars.

The result of the ballot was the election of twenty Associate Fellows out of the thirty-five names submitted by the Council.

On the instructions of the Council, the scrutineers of the ballot provided the following information, which the Council desires to be known at this Meeting :—

Only thirty per cent. of the total membership returned their voting papers.

Five voters crossed out over thirty names. Nine further voters crossed out between twenty and thirty names.

All candidates received five or more adverse votes.

Under these circumstances the Council feels justified in expressing regret that several eligible candidates should have been prevented from being included amongst the first Associate Fellows by the action of a small number. Had this action frustrated the creation

of a technical side, the Society would not now have an asset, which the Council believes will in course of time become the most valuable feature of the Society's constitution. It was the direct desire of the Society that a technical side should be created and the conditions under which the present Council assumed office caused that work to be regarded as their most important duty. In future, therefore, the Council hopes that members will not cast votes against the recommendations of the Council without full personal conviction that such action is just to the individual and to the Society.

At a meeting of the first Associate Fellows, the Council was requested to postpone the election of First Fellows in order that such an important event might be given the mature consideration it deserves. The Council need hardly remind members that Fellowship of the Aëronautical Society is the highest technical honour that the aeronautical world has to confer.

In the meantime, further elections to Associate Fellowship will be held as occasion warrants, but the First Fellows will in due course be elected from among the first Associate Fellows only, in accordance with the rules.

Meetings.—In addition to the routine of the Society's work, which has been rather arduous during the present session, the Council has organised a series of meetings for the reading and discussion of papers. All of these have proved extraordinarily successful.

In this work the Council desires to acknowledge the invaluable service so kindly rendered by Major-General Ruck. No one could possibly have worked harder or to better effect on the Society's behalf than has General Ruck during the last few months, and much of the success of these meetings has been due to his efforts.

Also, the Council desires to acknowledge the very valuable papers contributed by Colonel J. E. Capper, C.B., R.E., Major Radcliffe, Dr. E. H. Hankin, M.A., Sc.D., and Lieutenant C. M. Waterlow, R.E., and to express appreciation of the courtesy of Major-General R. M. Ruck, Sir George Darwin, K.C.B., F.R.S., Sir John Wolfe-Barry, K.C.B., and Colonel Sir Charles Watson, K.C.M.G., who have acted as Chairmen and thus assisted the Society to extend its sphere of interest.

In connection with several of these meetings the military authorities have most courteously facilitated the Society's work. Colonel the Rt. Hon. J. E. B. Seely, Under-Secretary of State for War, Major-General Sir C. Hadden, Master-General of Ordnance, and Lieut.-General Sir Arthur Paget, have all supported the Society by attending, and although it has not yet been possible to organise an essentially naval paper, H.S.H. Prince Louis of Battenberg has, nevertheless, very graciously shown the interest which that branch of the Service takes in the Society's work, by honouring one of the meetings with his presence.

The Royal United Service Institution has also been extremely considerate towards the Society in connection with the use of their admirable theatre.

Informal Meetings.—A new interest in the Society's life is to be developed by holding informal discussions in the Society's rooms on Monday afternoons at five o'clock. The Council asks for energetic support in making this move a success ; it has been inaugurated for the benefit of those interested in technical detail, and can only be maintained by their active co-operation.

New Premises.—Realising that the Society's work was handicapped by the inadequate accommodation previously afforded in Victoria Street, the Council decided to acquire new premises, and eventually sanctioned the lease of a suite of rooms at 11, Adam Street, Adelphi. In the work of selection and decoration the Council desires to recognise the personal service of Mr. B. G. Cooper, who is very largely responsible for the comfortable and attractive character of the Society's present headquarters.

Official Notices.—As the AËRONAUTICAL JOURNAL is published only once a quarter, arrangements have been made whereby the official notices of the Society will henceforth be published weekly in THE AEROPLANE, THE CAR ILLUSTRATED and FLIGHT, and monthly in AERONAUTICS.

This will enable the Society more effectively to make known its work to members and others, and will be a source of convenience to all as well as some saving of expense.

Research.—It has been impossible, both for lack of time and funds, for the Council to give special attention to this branch of the Society's work, but under the able supervision of Mr. A. P. Thurston, a series of tests made on behalf of the Society has lately been concluded and the remaining reports on this work will shortly be published. In this work the Society is indebted to the East London College for its co-operation and the use of its laboratory, and to Professors D. A. Low and J. T. Morris for their kind assistance.

Colonel Fullerton is also pursuing his very interesting investigation of bird structure, in continuation of the able work that he recently published under the Society's ægis.

Future Programme.—As far as possible the present Council has endeavoured to utilise their time in making a clear field for the new Council about to take office, and have therefore concentrated attention on bringing to a successful issue the special lines of action reported on above.

The present Council holds the view, however, that the systematic organisation of frequent meetings and discussions is a vital part of the Society's work, and has prepared the ground accordingly in the hope that this same policy may continue to be pursued. During the next session it should be possible to present a programme of unique interest and value.

The Council also desires to urge the advisability of devoting special attention to the organisation of the Students' section, for which provision is made in the Rules. It has hitherto been impossible to find time for this work, nor was it deemed desirable to commence until the Society was in possession of its new premises.

For the rest, the following *resumé* of the various committees and their duties will serve to show what is afoot:—Inventions Committee (which deals with inventions submitted for the Society's opinion); Library Committee (which has charge of the Society's Library and records); Publications Committee (which controls and organises the Society's publications); Relations Committee (which treats, when occasion arises, with other aeronautical bodies); Research Committee (which conducts the research work of the Society in all its branches). Representatives have also been appointed on H.M. War Office Technical Reserve Committee, the Parliamentary Committee on Aeronautics, and the Royal Aero Club Committee for the investigation of Aeroplane Accidents.

Finance.—The Society is in a more promising position, but the Council strongly urge upon members that it is imperative they should pay their subscriptions and that they should use their best endeavours to enlist recruits.

The Society's Position.—The Society's position in the realm of Aeronautics is one of unquestioned importance, which is bound to increase. It is essential, as indeed it is the common practice in all industries, for the scientific side of a movement to be under the ægis of a professedly scientific society, the members of which in each generation are responsible for the preparation of the knowledge that its successors shall ratify.

The Aëronautical Society of Great Britain should be proud to remember that it is the oldest Institution of its kind in the world, and all who have the interest of aeronautics at heart should work hard to maintain its prestige. At the same time, other interests must be recognised and supported; and the Council considers that the maintenance of the harmonious relations that at present so happily exist between the Society and the Royal Aero Club is a matter of the utmost importance to the welfare of the movement that it is the purpose of both bodies to encourage.

Mr. A. E. Berriman said:—Gentlemen, our next duty is to move the adoption of the Council's Report and Balance Sheet for 1911. You have all had copies of both in

your possession. It is unnecessary for me to do more than merely to refer briefly to one or two of the principal items contained therein. You will notice that the finances of the Society are now managed by a Company which differs from the Society merely in name: the name is "Aerial Science, Ltd.," and that is merely to keep the Society clear of any word like Limited. The Board of the Company is the Council of the Society for the time being, and it is impossible for any monies made by the Company to be utilised in any other manner than for the Society. It is exactly as if the Society were doing it in its own name, only it keeps the name free from the word Limited; and it is a means of limiting the liability of members to the amount covered by their subscriptions.

The next point that you will notice on the Report is one relating to the creation of Associate Fellows. I wish particularly to draw your attention to the report of the Scrutineers as therein contained. It is a little necessary to draw attention to that fact because it was the decision of the last General Meeting that the Society should have this technical nucleus, and the work of arranging for it was entrusted to the Council. This having been done, the Council think it was not quite proper for votes to be cast against people merely because they were not known personally: and there are indications that such was done.

In connection with meetings, there was rather insufficient time to organise quite as many as we should have liked, but those that have taken place have been very successful. In particular I wish to draw attention to the informal meetings that have taken place at the Society's Rooms at 11, Adam Street, Adelphi, on Monday afternoons. They appear to be becoming successful. Any member can turn up there and discuss the subject that happens to be down for the afternoon, and can also get tea in the rooms. The new premises are more convenient in many ways than the old rooms in Victoria Street. We have now a suite of offices, and the principal room for the use of members is quite comfortable.

Another very important point is that connected with official notices. It will be more convenient, and it has been decided to publish the official notices in the principal technical journals. All members will naturally be in the habit of taking in one paper or the other, consequently there will be no need to send out notices as heretofore by post. That will save us a certain amount of money, in fact, a considerable amount of money, and we need all we can get.

Research is proceeding in the ordinary way: nothing very special has been done by the Council in that connection. The Council desires to emphasize that the real vital work of the Society is the holding of really good meetings and discussions, really bringing out the scientific information that is available at each period of aeronautical research, and they hope that all members will come forward and assist as far as possible by giving papers and writing theses for publication.

You will observe from the Balance Sheet that the finance is in a satisfactory condition. We are able to continue, but we need more members and we certainly need money, but nevertheless it is in a fairly healthy condition. I might refer just to one little detail. Mr. McClean made us a loan in order to cover current expenses during the time that "Aerial Science" was in formation. We could not obtain the money that was in the bank until "Aerial Science" was properly formed, consequently we had that small loan which has now been repaid. I think there is nothing further that need be said in this matter, as you have the whole of the Report before you. I will therefore formally move the adoption of the Report and Balance Sheet and ask someone to second that in order that it may be discussed if necessary.

Major Sir Alexander Bannerman, Bart., C.B., R.E.: I will second it.

The Report and Balance Sheet were then submitted to the vote and unanimously adopted.

Mr. A. E. Berriman: The next business is to put the proposed amendments and alterations of the Rules of the Society before the Meeting. You have all had the notice

paper giving the amendments and alterations of Rules. The Council do not propose to proceed with all of these. I will therefore take only those that it is proposed to proceed with.

Rule 11. Insert (after "every Voter") "*resident in the United Kingdom.*"

Rule 11 relates to the matter of the Annual General Meeting, and it is a mere formality to put "the United Kingdom," for otherwise if anyone is abroad we cannot give the suitable notice. I will therefore formally move that Rule 11 be amended as stated on this paper.

MR. T. W. K. CLARKE: I will second it.

The alteration was submitted to the vote and unanimously adopted.

Rule 18.

MR. BERRIMAN: *Delete Rule* 18. Rule 18 relates to the election of the Council for 1911. It is no longer necessary to have that particular special rule in the printed list, consequently whilst we are reprinting we might just as well leave out what is redundant. I will therefore formally move that Rule 18 be deleted.

MR. CLARKE: I second it.

The motion was submitted to the vote and unanimously adopted.

Rule 30.

MR. BERRIMAN: *Delete Rule* 30. This relates to members of the Society elected to Associate Fellowship at the first ballot to pay a fee of £2 2s. That is already finished, and we want to put in place of Rule 30 another Rule; that will save us having a Rule 31b or anything of that sort. We will take that out and make room for another Rule. Substitute "*Members residing abroad shall pay no entrance fee upon election, and shall pay an Annual Subscription of* £1 1*s. only.*" We have a number of applications from abroad, and felt it was desirable that they should have some facilities. It is a very common practice in all societies to give special facilities to those abroad. It is therefore proposed that they shall pay no entrance fee and shall only pay an annual subscription of £1 1s. I will move an amendment of the Rule to that effect.

MR. CLARKE: I second it.

After a short discussion the amendment was submitted to the vote and adopted.

Rule 31.

MR. BERRIMAN: *Delete Rule* 31. This rule relates to new members joining the Society between 1st November and 31st December, 1911. It is no longer necessary. I will formally move that it be deleted.

MR. CLARKE: I second it.

The motion was submitted to the vote and unanimously adopted.

Rule 33.

MR. BERRIMAN: *Substitute for* "1911," "1912," *in Rule* 33. That is, "Students joining the Society during 1911 will be admitted without entrance fee." The reason for increasing the time limit is that we have not had time to attend to this student problem, and therefore it is proposed to extend the privilege of joining without entrance fee during the year 1912, when we hope to make a general attack upon the students to get them into the Society. I therefore formally move that.

MR. CLARKE: I second it.

The motion was submitted to the vote and adopted.

Rule 34.

MR. BERRIMAN: *Delete "except those claiming under Rule* 30." Rule 34 relates to members transferred to the technical side. We have now to delete the words "except those claiming under Rule 30," because Rule 30 has been altered. It is mere formality, and I move that those words be deleted from Rule 34.

MR. CLARKE: I second it.

The motion was submitted to the vote and adopted.

Rule 35.

MR. BERRIMAN: *Add "Should a Life Member resign or be expelled from the Society no part of his life composition paid by him shall be returned."* It is merely a formal addition to that Rule which might be useful to have in the Rules. I move it.

MR. CLARKE: I second it.

The motion was submitted to the vote and adopted.

Rule 51.

MR. BERRIMAN: It is proposed to *delete Rule* 51. "The President, Secretary, and Librarian shall have the superintendence of the Library and other like property, and shall be a permanent Committee for that purpose." The President for the moment does not exist, and the Secretary and Librarian are one and the same person. We have a Library Committee which we deal with in the next clause which should secure us if we delete the Rule. I move that Rule 51 be deleted.

MR. CLARKE: I second it.

The motion was submitted to the vote and adopted.

Rule 52.

MR. BERRIMAN: For "*the above Committee*" substitute "*a Library Committee.*" Rule 52 says, "A catalogue of the books presented, or purchased by the Society, and of all instruments, models, manuscripts, tables, and other like property, with the names of the respective donors shall be kept under the superintendence of the above Committee." All we need to add there is a Library Committee and leave the Council to form a Library Committee. As a matter of fact it has already been formed. It therefore brings us into line. I formally move that Rule 52 be amended as proposed.

MR. CLARKE: I second it.

The motion was submitted to the vote and adopted.

Rule 54.

MR. BERRIMAN: For "with leave of the Librarian or person acting as such for the time being," substitute "*subject to the regulations for the time being in force.*" This relates to the use of books. We cannot have the promiscuous removal of books from the Library now we have rooms in which to inspect them. It is very awkward to have the books you want removed from the Library, and it is therefore proposed that the removal of books from the Library shall be subject to regulations. If the Council choose to make regulations for the time being that the books cannot be removed from the Library, then this Rule enables them to do that. I move it.

MR. CLARKE: I second it.

The motion was submitted to the vote and adopted.

MR. BERRIMAN: Those are all the amendments that it is proposed to proceed with. There are other Rules of which notice has been given, and should anyone desire to move any of these further amendments I should have to rule it in order, but they are not proceeded with by the Council.

MR. B. G. COOPER: I wish to move the section relating to polls of the whole membership.* The principle underlying that is really the principle underlying the ballot which we will hear declared this evening, that is, the members of the Society shall manage their own affairs. In other Societies, such as the Institution of Civil Engineers, which is an eminently respectable Society, this provision exists. Twenty corporate members can call for a ballot of the whole of the corporate members residing in the United Kingdom. In the Cyclists' Touring Club, a Society with a membership of over 20,000, a similar section exists, and it has been proved useful, and the section which I have down here is founded upon that of the Cyclists' Touring Club and on that of the Civil Engineers. If you accept the principle which is as a matter of fact already accepted, because the principle is embodied in our present rules, and this is merely an extension of it, I ask that you agree to the insertion of this section in our Rules. I won't say any more except that we cannot do better than follow the example of the Civil Engineers in a case like this, I formally move that section "Polls of the whole Membership," as printed on this list, be inserted, and I shall be glad if someone will second that.

COLONEL H. E. RAWSON, C.B., R.E.: I second that with pleasure.

MR. BERRIMAN: It has been formally proposed and seconded that Rule 43 be amended in the form stated on the Paper.

THE PROPOSER: I should like to say that where I have said "to a poll of the whole of the voters," I mean a poll of the whole of the voters resident in the United Kingdom, as is provided further up in the Rules.

MR. B. WOODWARD: I only wish to point out in regard to this that I do not think you will ever come to a conclusion. You hold a meeting and you take a vote of the meeting, and then it is capable for any 20 members of the Society to disregard the vote of that meeting and call for a poll and so upset the whole arrangement, and that might go on from time to time, and you never come to any conclusion.

THE PROPOSER: It binds them for the rest of the current year. I say that this privilege has not been abused in the Civil Engineers or in the Cyclists' Touring Club. Objections might be raised on technical grounds, but I do not think they will arise in practice.

MR. BERRIMAN: Do you move an amendment?

MR. B. WOODWARD: I am afraid I have no amendment to move; I am only speaking to the resolution; I cannot controvert it by way of amendment, but it does seem to me that it is putting the Society to expense. The people who attend a meeting will generally

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*"*Polls of the Whole Membership.*—Any question may at any time be submitted by order of the Council or by resolution of a General Meeting, to a poll of the whole of the voters, in such manner as the Council or General Meeting shall direct.

"Any twenty members of the Society may call for a poll of the whole membership upon any question which shall have previously been before a General Meeting of the Society, and such poll shall take place within one month from the date of receipt by the Secretary of the letter calling for such poll.

"All decisions arrived at by a poll of the whole Society shall bind the Society and Council for six calendar months from the declaration of the poll, or for the remainder of the current year, whichever may be the longer period.

"Should it be determined at any meeting to take a poll of the whole of the members on any question, the question shall be clearly placed before the members either in the Journal or by separate communication, and any letter or manifesto on the matter at issue signed by twenty members shall be circulated in such Journal or communication, provided that it shall have been received by the Secretary seven days before the issue of such Journal or communication."

know something about the subject, and then you go to a poll and you go to people who have heard nothing about it and do not know the ins and outs, and perhaps it is overruled. They have not taken the trouble to attend the meeting and therefore in a sense they ought not to be consulted. I merely make those remarks.

The motion was submitted to the vote, when 6 voted in favour and 10 against, and the Chairman declared it rejected.

MR. BERRIMAN: Does anyone wish to move any amendments that have not been proceeded with?

(No response.)

In that case I will move that leave be granted to withdraw the other suggestions and amendments that we have not proceeded with.

The motion was adopted.

The Scrutineers then returned and delivered their report.

THE CHAIRMAN: The Scrutineers report as follows:—

128 papers were sent in, 5 disqualified, 123 papers effective. The result of the ballot on the technical side: the Associate Fellows elected to the Council are:—

Captain A. D. Carden, R.E.
T. W. K. Clarke.
J. W. Dunne.
J. H. Ledeboer.

The Members of the Council on the ordinary side:—

Colonel J. E. Capper, C.B., R.E.
Lieutenant R. Gregory, R.N.
F. K. Maclean.
Major-General R. M. Ruck.

I am sure that we owe a great debt of gratitude to the Scrutineers for having so thoroughly got through this work.

The Meeting then terminated.

GENERAL MEETING.

THE SECOND MEETING of the Forty-Seventh Session of the Aëronautical Society of Great Britain was held at the Royal United Services Institution, Whitehall, S.W., on 30th January, 1912, at 8.30 p.m. SIR JOHN WOLFE-BARRY, K.C.B., occupied the Chair.

SIR JOHN WOLFE-BARRY: Ladies and gentlemen, this meeting has been convened somewhat hurriedly, but your attendance justifies the Council in calling it, and ensuring an opportunity for the discussion of "Soaring Flight" before Dr. Hankin's urgent departure for India. Many of those who are present no doubt heard the interesting lecture which was given by Dr. Hankin upon the Flight of Birds, and our purpose to-night is to have some little further talk on the matter and especially on the most wonderful part of the flight of birds, namely, their power of soaring. Some birds, particularly those observed by Dr. Hankin, have developed this art of soaring to perfection. Before any discussion upon the matter of soaring takes place it will be of utility, I think, and help us in the discussion of the subject, if some general conditions and data are put together so that the matter may be discussed with some approach to definition of what is in the minds of different speakers, because no doubt the point of view is not always the same among those who have studied this very interesting subject. It was, therefore, thought to be a convenience if Mr. Berriman would undertake the preparation of an opening argument as a basis for further discussion. I will ask Mr. Berriman now to proceed to read his paper.

MR. A. E. BERRIMAN then read a resumé of the observations made by previous investigators into Soaring Flight including quotations from Darwin, J. A. Froude, S. P. Langley, F. W. Lanchester, Mouillard and S. E. Peal.

THE SOARING FLIGHT OF BIRDS

DR. E. H. HANKIN: I am fully aware that the kind appreciation that you have shown of my work on the flight of birds is due to the circumstance that what I have published consists chiefly of facts and only to a limited degree of my opinions about these facts. You are aware that in making my observations, I recorded what I saw without reference to any kind of preconceived idea. I counted nothing as observed unless I had written down an account of what I saw at the time, without waiting for time to think whether what I had seen agreed with accepted views as to what was possible or what was impossible.

I understand that it is admitted that soaring flight can not be explained by means of known upward currents. I quoted cases in which soaring birds showed skill in avoiding known upward currents and pointed out that it is a difficult proposition to defend that they have skill in finding other upward currents that are not known to exist. I have also brought forward instances in which in spite of an upward trend in the air in a steep ravine of 1 in 5, birds were obliged to indulge in flapping flight as soon as heavy cloud cut off the sun's rays.

I am under the impression that the rays of the sun must be the ultimate source of all the manifestations of energy in the atmosphere. In fact all known upward currents or upward trends are due to the heat of the sun. In my published papers I have given an account of small ascending currents due to the heat of the sun that I have designated as "heat eddies." I have discovered how to observe these heat eddies and to a certain extent how to measure their intensity. As a rule in fine weather soarability develops in the morning when the heat eddies have reached a certain intensity. But I have published a table of cases in which, owing to the presence of thin cloud, the heat energy of the sun was cut off and no eddies developed. But in these instances soarability developed at its usual time. I have not yet published two or three other cases in which the converse occurred. Heat eddies developed at the normal time, but soarability only developed after an interval. These facts indicate that the energy involved in producing soarability does not come from that part of the sun's rays that produces heat eddies.

Therefore, to hold that soarability is due to upward currents or upward trends involves an assumption of something new and unknown, so far as I can see, to as great a degree as does the assumption of "ergaer."

Looked at from the broadest standpoint there are only two possible theories of soaring flight. One is that we are dealing with a manifestation of kinetic energy. The other is that we are dealing with a manifestation of potential energy.

I propose to mention, as impartially as possible, various facts that appear to bear on these two possibilities.

(1) Firstly I have stated that on one occasion, when the air had become unsoarable after a succession of heavy showers, sound travelled unusually far. The noise made by the beating of the wings of vultures and adjutants could be heard at a considerable distance. In other cases I have noticed sound travelling far in the calm or nearly calm and unsoarable air after a storm. These facts obviously support the idea of kinetic energy. If soarable air is not homogeneous, and is filled with pulsating eddies or local currents, one would not expect it to conduct sound so well as when such eddies have vanished and when the air is consequently no longer soarable.

But why is it that this fact was not quoted by my opponents? Why is it that in trying to disprove the theory of "ergaer" they have not quoted a single fact in support of their views? The arguments I have heard consist chiefly of quotations from authorities who have been palpably misled by travellers' tales, and who in complete ignorance of the real facts of the case make dogmatic statements as to the impossibility of soaring flight in a horizontal wind or in an apparent calm. It is time we heard of a little more respect for facts and a little less respect for authorities.

As a matter of fact, these cases of sound travelling unusually far do not occur sufficiently frequently to have much bearing on the question. As a rule, in the evening, air near the earth becomes unsoarable without it being possible to detect any change in its power of conducting sound.

(2) Secondly, I have described a case of a cheel circling and gaining height to the amount of about 150 feet, when enveloped in thin cloud, and when in a current of air that was slowly descending at an angle of about 10° with the horizon. The air was travelling about 2½ miles an hour. Owing to the cloud being in small discrete masses the movement of almost every portion of it in relation to other portions could be observed. There was no recurved eddy of air under the lee of the hill that could account for the gain of height. Had such a recurved eddy existed it could have been easily observed.

(3) In Naini Tal soaring in thin cloud was frequently seen. It only occurred when there was a strong glare of light. If heavier cloud rolled up overhead, so that the glare diminished, soaring birds had immediate recourse to flapping flight or else settled. If the sun energy, that is the source of the energy manifestation of soaring, has the property of penetrating thin cloud, one would not expect it to be reflected from cloud. A few observations in Agra and in Naini Tal show that a glare of light due to reflection from a large cumulus cloud is not accompanied by soarability.

In these cases of soaring in thin cloud, the cloud, generally, was in small discrete masses. These showed practically no relative motion. There was a complete absence of any eddy movement. The facts described in this and the preceding paragraphs show that if soarability is due to moving masses of air, the movement must be of a very extraordinary character. The facts obviously are more in favour of the potential energy theory of soarability.

(4) The hill crows in Naini Tal only soar in bright sunshine and in the complete absence of wind. Another observer has noticed the same fact as to the favouring influence of the absence of wind on the soaring flight of these birds. Therefore, on the kinetic theory the motion involved must be one that has nothing to do with wind. The interesting summary we have heard of what is known of soaring flight is obviously of historical interest, but in so far as it involves references to wind it has no connection with real soarability.

(5) I have published numerous cases in which soaring flight was observed above or below feathers or pieces of thistledown floating in the air which showed no tendency to gain height. It is conceivable that a feather might be dropping in wind that had a vertical component of two or three feet per second. I understand that it is asserted that, if wind had such a component, soaring flight could be explained. Unfortunately the kind of soaring flight that would be explained is of the ideal variety having but little relation to facts. In real soarability the bird is not only supported and driven ahead at 30 to 40 miles per hour, but also (in flex-gliding) the wing tip feathers are bent upwards, with a considerable degree of force. In the case of the vulture the force required to bend the wing tip feathers to a position similar to that seen in flex-gliding is as much as can conveniently be exerted by one hand. A vertically ascending wind having a speed of thirty miles an hour might perhaps be sufficient. The assumed vertical component of 2 or 3 feet per second is obviously incapable of producing the observed effect. It has been suggested that the turned up position of the wing tip feathers in fast flex-gliding might be due to the known lifting action of a horizontal current of air on a convex surface having the convexity upwards. Unfortunately for this suggestion the ends of the feathers are so far lifted up that their convexity is below. Thus in this case again the ideal soaring flight that is explained by the kinetic energy theory differs from the real soaring flight that can not be so explained. Therefore if soaring flight is due to kinetic energy the movement must be of a kind that has nothing to do with wind, that has no visible effect on small cloud masses, and that leaves a piece of thistledown apparently unaffected.

(6) The uniformity of soaring flight gives a strong proof that the energy involved is not due to any irregular and chance ascending currents. If a group of circling cranes is watched through binoculars, the birds are seen to keep their relative distance with marvellous regularity. The appearance is as if a number of dead birds was pinned on to a wall, all in the same position. Therefore all the birds must be taking energy from the air at the same rate. Therefore if the source of energy is due to ascending currents, these ascending currents must be small and uniformly distributed. I have found that small and apparently uniformly distributed ascending currents are caused by the heat of the sun. But I have given the strongest proof possible that these ascending currents are not the cause of soarability. Therefore on the kinetic theory of soarability there must be one set of ascending currents, small and uniformly distributed, that does not cause soarability, and another set of small and uniformly distributed currents that does cause soarability, and that has not yet been discovered.

(7) Birds can change from slow to fast flex-gliding by two different adjustments. If one adjustment, namely, increase of wing flexing and relaxation of secondaries is employed, the increase of speed takes place gradually. The speed appears to slowly increase during several seconds. If the other adjustment is used, namely a double dip with increase of flexing during the up-stroke of the double dip, the increase of speed appears to be almost instantaneous. The amount of increase of speed thus produced may be as much as 10 metres per second. On the kinetic theory the double dip should result in a dive of about 6 metres lasting for at least a second to produce this increase of speed. As a matter of fact the double dip does result in a strongly marked rotation round the transverse axis. But I have no recollection of seeing any well marked loss of height. On the " ergaer " theory, apparently the inclined position of the wings during the double dip results in increased air disturbance and so initiates an increased amount and rate of decomposition of " ergaer."

(8) On the theory that the air in a wind is moving forty different ways at once, any sign of instability shown by a bird is a matter not worth investigation. It is only what one would expect to see, signs of instability, if the air is full of varying and inexplicable currents. Fortunately, being uninfluenced by this theory, I have subjected cases of instability to investigation with results that appear to be of importance. The first case of instability that I observed was the lateral instability sometimes shown by the Black Vulture. The first clue to its nature was obtained, when, going over my records, I found that it only occurred between three and four o'clock in the afternoon. Later observations have shown that it may occur rarely at other times of the day, especially

in disturbed weather, but in such cases is not usually so well marked. The second clue to its nature was obtained when I realised that it is never seen in a wind in which birds show signs of transverse axis instability. This statement is based on a large number of observations and applies to cases of lateral instability seen in the early morning besides at its more usual time late in the afternoon. Though I have not yet seen lateral and transverse axis instability in the same wind, I have reasons for expecting to see them together in the " displacement wind " of a dust storm as will be explained later.

I believe lateral instability is due to an irregular distribution of " ergaer " for the following reasons :—

A. It usually occurs in the afternoon at a time when the soarability of the air is decreasing.

B. It only occurs at low levels, perhaps up to a height of 50 metres as a maximum, that is to say, in regions where the air is likely to be losing " ergaer " by friction with the earth.

C. It only occurs in the case of birds of heavy loading. The birds may be regarded as skating on thin ice. If one wing is not sufficiently supported it goes through sufficiently to make a lateral lurch.

D. In a wind in which a heavy bird, such as a black vulture, shows lateral instability, a bird of light loading, such as the cheel, shows dorso-ventral axis instability. To carry on the analogy, the bird of light loading does not go through the thin ice, but one wing is not driven ahead by decomposition of the " ergaer " so much as the other. Hence slight turns round the dorso-ventral axis occur.

E. The correctness of this interpretation is shown by the fact that a bird of intermediate loading, in such a wind, shows both dorso-ventral and lateral instability. I have seen this in the case of certain eagles.

F. The correctness of the interpretation is shown by the following observation made at Jharna Nullah on the 7th December, 1911, late in the afternoon. Soarability at low levels was decreasing. Birds at such levels occasionally employed flapping flight. Common vultures and black vultures frequently showed lateral instability. I happened to observe closely a vulture that was gliding in a direction away from me at a height of probably less than 10 metres above the earth. I watched it while it glided nearly in a straight line for several hundred metres. During this time there were three or four slowly developing and well-marked changes of the angle of incidence. Apparently in the presence of more " ergaer " the bird glided at higher speed with a smaller angle of incidence. When it got into a patch of air having less available energy it glided with a higher scale of incidence and at lower speed.

I do not wish to say that the kinetic energy theory is excluded by these observations. But it is obvious that the kinetic energy theory can only explain the facts with difficulty. A simple explanation, on the other hand, is furnished with the help of the conception of " ergaer."

(9) We now have to consider " tail-jolting winds," that is to say, winds in which birds show evidence of transverse axis instability. This kind of instability has the following characters :—

A. It occurs at as high levels as can be observed, besides at low levels.

B. It occurs in the early morning, and during the day at a time when the air has full soarability.

C. In certain cases I have been able to observe the actual tipping up round the transverse axis. The bird appears to be suddenly rotated upwards round this axis to an extent of perhaps as much as 45°. If this tipping up was due to the bird gliding into an ascending current, then, occasionally at least, it should meet an ascending current with one wing and not with the other. This apparently never happens in ordinary winds. I have never yet seen signs of lateral instability in a tail-jolting wind.

D. If the tipping up round the transverse axis was due to an ascending current, then, assuming the bird to have a speed through the air of 8 or 10 metres per second, the bird would obviously be completely involved in the up-rising air before it had time to commence an adjustment. The up-rising air would bring backwards the centre of lifting effort of the wings, that is to say, the bird would have to combat the tipping up by a retirement of both wings. This it certainly does not do. On the other hand, in extreme cases, the bird combats the tipping up by the following adjustment. The tail is elevated; both wings are placed dihedrally down (to produce rotation downwards round the transverse axis); and there is an increased relaxation of the secondaries. That is to say, the bird makes the adjustment for bringing backwards the centre of lifting effort of the wings, and therefore the tipping up must be due to the bird getting into a patch of air in which the lift moves forward. Obviously, on the theory that energy of soaring flight is derived from "ergaer," the "ergaer" must decompose at a certain rate. The rate is such, that in flex-gliding the pressure from "ergaer" is, on an average, exerted at a point near the centre of area of the wing, as I have shown elsewhere. If, as in the case in question, the pressure is suddenly exerted at a point nearer to the anterior margin of the wing, then this can only be due to the "ergaer" being more than usually unstable. I have published a case of transverse axis tipping up caused by a bird passing *gradually* into an ascending current.

Thus the phenomena of transverse axis instability, so far as I have at present described them, appear to be inexplicable on the kinetic theory of soaring flight. They can only be explained on the potential energy theory by making the further assumption that the liability of "ergaer" to decomposition can vary owing to some influence as yet undiscovered. Further facts bearing on this possibility will be described in the following paragraphs.

In a tail-jolting wind cheels commonly show from one to two small tail-jolts per second. Larger jolts occur at intervals of several seconds.

(10) I have now to describe a phenomenon that I propose to term "wind soarability." Most of my published work on soaring flight has related to "sun soarability." I have, however, mentioned briefly "disturbed weather soarability" and "storm soarability." In the monsoon season of 1910, I failed to obtain any clue to the nature of disturbed weather soarability. But during 1911, I had the opportunity of observing the change from sun soarability of the hot weather to the disturbed weather soarability of the monsoon season. During April and May of that year it usually happened that sun soarability developed at its normal time and coincided in time of development with the appearance of heat eddies on the fort bastion as elsewhere described. But on certain occasions, from half an hour to an hour and a half before the development of sun soarability, local and temporary soarability was observed. Such local soarability always coincided with the appearance of a gust of wind. Also, especially at the commencement of this kind of soarability, the cheels showed a high degree of transverse axis instability. That is to say before the available sun energy was sufficient, and in the absence of wind, air was unsoarable. But in the presence of a gust the air became soarable and became unsoarable as the gust died away. From the evidence of transverse axis instability we may infer that a gust of wind is an area in which "ergaer" shows an abnormal liability to decomposition. Perhaps it is spontaneously decomposing and thus causing the puff of wind. During the monsoon season I found that in the absence of sunshine or sufficient glare of light, the air was always unsoarable except in the presence of wind. In the absence of sunshine the wind is frequently, but not always, soarable. At various seasons of the year I have observed that so long as the wind is steady cheels fly steadily, but that they show tail-jolting during a puff of wind. It will be convenient to regard "disturbed weather soarability" and "storm soarability" as special cases of "wind soarability."

Wind soarability differs from sun soarability in the following points:—

A. Sun soarability commences at a definite time of day, varying with the season of the year from about 6.30 in June to about 9.30 in December, and is demonstrably

connected with the amount of energy present in the sun's rays. The bird of lightest loading, the cheel (·55 lb. per sq. foot), is the first to commence soaring. Birds of other species start at later times in the order of their loading.

Wind soarability may occur at any time of the day, and in the absence of sunshine. I have seen it (in connection with " displacement winds " of dust-storms) shortly after sunrise and long after sunset.

B. In sun soarability cheels usually, or perhaps always, commence soaring over the houses of the city of Agra a few minutes before they commence over the trees and gardens of Agra Cantonment.

In the case of wind soarability no such relation occurs. Cheels begin circling wherever there is wind.

C. In sun soarability air at low levels appears, in some cases, to become soarable before air at higher levels. That is to say, if a cheel circles to too great a height (at the time of commencement of soarability), it may find itself in air in which it has to flap.

In the case of wind soarability there is a tendency for wind to be more soarable at higher levels.

D. Sun soarability, as instanced by the hill crows, not only occurs in the absence of wind, but is favoured by the absence of wind.

Wind soarability only occurs in the presence of wind.

E. In the case of sun soarability there is no evidence of any particular degree of instability at its commencement.

In the case of wind soarability, instability is frequently noticed especially at its commencement. My observations do not allow me to venture an opinion as to whether or not wind soarability is always characterised by transverse axis instability.

F. In the case of sun soarability birds only soar at a certain distance above the earth. In the case of cheels over a flat plain perhaps the limit of distance is at a height of two or three metres. For vultures, when the air is fully soarable, the limit of distance may be between five and ten metres. This opinion is a guess not based on definite measurements.

In the case of wind soarability, especially in displacement winds, cheels may get energy from the air however near to the earth. In a strong wind of this nature I have seen a cheel, with its wings in the slow flex-gliding position, pick berries off the upper twigs of a neem tree as it passed without interrupting its gliding flight. In such winds cheels commonly glide over my house, at a height of two or three metres above it, without showing gain of height from the ascending current that must have existed on its windward side, or loss of height in the absence of such ascending current as they glided up towards the house from its leeward side. On one occasion during a stormy displacement wind I watched more than a hundred cheels gliding up wind, some at a low level just clearing the house and others at heights up to perhaps 200 metres. Whether at low level or high level they glided against the wind making about a walking pace over the earth. The wind was so strong that it was somewhat difficult to stand up against, and when I happened to get up from my chair, the latter was instantly blown to leeward across the roof. Within a few yards of me cheels were gliding up wind, with wings in the slow flex-gliding position, but making speed ahead through the air probably far in excess of their speed in sunshine in fine weather.

(11) I now propose to state briefly the results of my many years' observations on the development of dust-storms. If my conclusions are correct, it will be seen that the facts of the case exclude the idea that soarability is due to kinetic energy.

In the first place it may be stated that every possible intermediate form of disturbance exists between a small dust-storm and a large thunderstorm with heavy rain. In the case of the larger storms, especially if rain falls at more than one point, it is impossible in my experience to find out anything about the nature of the changes of wind and wind direction.

Statements have been made that thunderstorms consist of small cyclonic disturbances, and it appears to be suggested that the wind of a thunderstorm travels in a circular direction. I may point out that if these statements are based on facts of observation, they presumably are based on facts observed under unfavourable conditions.

Supposing one is observing the development of a small isolated dust-storm occurring in otherwise fine weather, the following phenomena may be observed :—

A. The night has been cloudless and calm. During the day small bun-shaped cumulus clouds, situated at somewhat regular intervals, have been travelling across the sky. There has been a light wind, moving small branches, that acquires its strength (at the level of the earth) two or three hours after sunrise. This wind may be called the " real wind." The clouds began to form at a varying interval after sunrise. At first small wisps of cloud develop whose margins are seen to be dissolving. But the central part of the cloud is seen to be rising and having the characteristic cotton-wool mass shape of cumulus. At first these shapes are mere outlines and nearly transparent. They rapidly become opaque and enlarge in size to form the characteristic cloud form. There can be no doubt that these changes are due to sun heat being absorbed in the lower moisture layer of the atmosphere. The heated air rises and condenses. Round the edge of this condensing mass of cloud there is a slight descent of the air. Hence the margins of the developing cloud are seen to be dissolving as small portions of the cloud-bearing air descends to warmer levels.

B. During the afternoon the first sign of a storm may be seen in that a single cumulus cloud appears to lose its speed ahead to some extent. It commences rising. Other clouds coming from windward fuse with it. Hence to leeward of it there is an unusually large area of blue sky. I have frequently been warned of the coming of a dust-storm by noticing this large area of blue sky.

E. The above described cloud mass is the developing storm. It has a speed always less than the real wind. It may travel nearly in the direction of the real wind. Or it may diverge from the real wind direction to a greater or less extent.

D. As the cloud mass rises, forming the " turret tops " frequently observed in thunderstorms, the air appears to be attracted in all directions towards it. That is to say on the leeward side of the storm, for instance, the real wind drops to a calm, and then is replaced by a wind that blows in the direction of the storm. This wind may be called the " attraction wind." Owing to this attraction wind the general public, that never learns by experience, is so frequently caught out in a dust-storm. One hears the remark " I don't think the storm is coming this way because the wind is blowing it away from us." A few minutes later the wind changes by 180°, and the storm arrives. On the windward side of the storm the " real wind " and the " attraction wind " coincide in direction. At the sides of the storm the wind travels more or less directly towards the storm centre, it may be presumed, accordingly as the " real wind " or the " attraction wind " preponderates. My observations do not exclude, but fail to give any evidence of, cyclonic movement. The wind appears to blow, at ground level, in all directions towards the storm centre. The cloud mass is rising. That is to say, as the " attraction wind " approaches the storm centre it acquires an upward trend and then rises vertically or nearly vertically.

E. The height that the top of the cloud mass reaches varies in different cases. Occasionally it reaches the cirrus level. In such cases the top of the cloud mass may be seen to push the cirrus layer up above it. A small space is always visible between the top of the cloud mass and the displaced cirrus. That is to say, the cirrus forms a small cap over the top of the cumulus. If the sun is suitably situated this cap may show beautiful iridescent colouring. It may be noted that this observation relates to comparatively fine weather. In some cases and especially if there is a tendency towards wide-spread rain, the cirrus layer is denser and fuses with the rising cumulus. But in such cases there may also be a layer or layers of cloud intermediate in height between the low level cumulus and the high level cirrus, and the phenomena are more difficult to follow.

F. The whole of the cloud mass has and retains the characteristic cotton-wool like appearance. There is no sign of air or wind being taken in at any point from outside.

G. After the "attraction wind" has been blowing for a variable time, a descending current of air appears to form in the centre of the cloud mass. This air descends until it strikes the earth and then radiates in all directions. It thus forms a wind that I propose to term the "displacement wind." This wind is strongest on the leeward side of the storm. It may blow to a limited distance on the windward side. The fact that it blows more on the leeward side of the storm appears to suggest that the descending current from which it arises does not travel vertically downwards but that it is inclined obliquely in the direction of the storm path.

H. While the "displacement wind" is blowing at ground level, the "attraction wind" still continues to blow towards the storm above it. I have seen the smoke from a factory chimney blowing away from the storm (from "displacement wind"), then as it gained a height of a few feet above the top of the chimney, it was seen to turn through 180° in the vertical plane and blow towards the storm (*i.e.* in the "attraction wind"). I have observed this on several occasions. As the storm approaches, the depth of the "displacement wind" rapidly increases.

I. The "displacement wind" has a remarkable power of picking up dust, thus forming the dust-storm. In some cases the dust may be seen advancing as a dense yellow wall of dust, one or two hundred metres behind the front limit of the "displacement wind." In other cases the dust is more diffused.

J. The "displacement wind" may feel hot if it is not associated with rain. It feels cold if the storm is associated with rain.

K. It may be accompanied by a few flashes of lightning. If there is rain and if thunder is heard, especially if heard clearly, this is a sign that the storm is receding.

L. In rare cases when the dust is followed by heavy rain, the "displacement wind" may contain pieces of cloud of very peculiar shape that I have termed in my notes "smoke-pattern cloud." Having had some experience of estimating the height and sizes of clouds by measuring the heights at which vultures were gliding in and out of small cumulus clouds in Agra, I may perhaps hazard a guess that these smoke-pattern clouds travel at heights of between one and four hundred metres above the earth. It is a matter of common knowledge that smoke enters in a continuous stream at the bottom of a factory chimney and comes out at the top in puffs. The explanation given is that the column of air in the chimney is in a state of slow vibration with a wave length primarily equal to that of the chimney. If this is the case it is possible that the appearance of smoke-pattern clouds may be due to a similar cause. The supposition that they are due to condensation does not explain their extraordinarily smoke-like appearance.

I have seen smoke-pattern clouds issuing from the centre of a storm in two directions making an angle of about ninety degrees with one another. Shortly after noticing this, other smoke-pattern clouds advanced over me with torrents of rain, bringing my observation to a close.

Obviously the above observations give but little clue to the nature of the processes going on inside the storm cloud. The simplest supposition is that the air of the "attraction wind" having travelled upwards for some distance inside the cloud turns back and descends at the centre of the storm forming the "displacement wind." The suggestion that the "displacement wind" is caused by the fall of drops of rain does not appear to agree with the fact that dust-storms without rain frequently occur, unless we may assume that rain is formed and evaporated before it reaches the earth.

The interest of the above account of the development of dust-storms lies in the changes of soarability that are connected with them.

If the sun is shining the "attraction wind," so far as I have observed, remains soarable. But if the sunshine is cut off by cloud, the "attraction wind" may completely lose its soarability. In one case I observed a progressive decrease of soarability as the storm approached. At first cheels circled, but had no power of flex-gliding up wind. For going up wind they used "lift-gliding" with gain of height. A few minutes later this was replaced by lift-gliding without gain of height, or by flap-gliding.

Two suggestions may be made to account for the loss of soarability of the "attraction wind." Firstly, it is possible that, under the influence of the storm, the "ergaer" decomposes, perhaps forming the wind. But if this were the case, one would expect transverse axis instability to be observed. But this I have not noticed.

Hence there is no evidence of increased rate of decomposition of "ergaer." The alternative view is that under the influence of the storm it became too stable to be useful for purposes of soaring flight.

The "displacement wind" on the other hand is, nearly always at least, highly soarable. Frequently when soaring in this wind the birds show very strongly marked transverse axis instability. That is to say there is reason for believing that in the "displacement wind" "ergaer" is unusually unstable.

If my view is correct that the same air passes from the "attraction wind" into the "displacement wind," then air may lose and then gain in soarability. It is impossible to see how this can happen on the kinetic energy theory of soaring flight.

In conclusion I may refer to two common observations that I have heard quoted as evidence that air is full of streams and eddies of incalculable and unknown nature. Firstly, the appearance of smoke issuing from a chimney is stated to be due to such eddies. But if a pinch of coarse sand is dropped into a glass of water the particles do not drop vertically to the bottom. The particles are of different sizes. The currents caused by the larger particles deflect the smaller particles from their course. If finer sand is used a further distribution of particles takes place. If fine liquid mud is dropped into the water it spreads out into masses exactly similar to those produced by smoke issuing from a chimney. That is to say in this case smoke forms are produced by the action of the particles on one another either direct or by the streams and eddies that they produce. The smoke forms furnish no evidence of the presence of eddies pre-existing in the water. Secondly, the flapping of a flag in the wind, or of clothes hanging on a clothes line may give a misleading impression of the complexity of wind. If a piece of paper tape is tied on to a kite string, and if near it a piece of thread is attached to which are tied one or two light downy feathers, then, on sending up the kite, the tape is seen to be in irregular flickering motion, while the feathers remain nearly motionless at the end of their horizontally stretched out thread. If a portion of the tape happens to turn in such a direction that the air impinges full upon it, then it is moved, and in moving brings other portions of the tape to positions in which they are more or less acted on by the air. Therefore each portion of the tape is subjected to the force of the air to different degrees at different moments. Hence the flickering appearance, which gives no real index to the motion of the wind. Probably in the "displacement wind" of a dust-storm, especially perhaps near where it strikes the earth, the feather on the thread would show highly irregular movements, but this is no proof of the existence of similar movements in wind on a fine day and in the presence of sun soarability.

MR. W. G. ASTON: I do not propose to propound any theories. I have come to hear what other people have to say; but if I might be allowed to, I should like to ask Dr. Hankin one or two questions with regard to his theory of "ergaer." The first one is, has he taken any records of the humidity of the atmosphere when birds, as he states, have been able to soar in absolutely calm air; has he made any notes on the effects of local thunderstorms upon the birds, the capability of soaring under such conditions. Next, does he suggest that both light and heat are essential for the production of "ergaer," in other words, is it conceivable that a bird could soar on a day that is merely warm in calm air, or is it conceivable on the other hand, that it could soar in moonlight and can get no warmth, but light. I want to know what happens to "ergaer" after the energy has been extracted

from it by a bird's wing; does he suggest that sunlight returns the energy to "ergaer" immediately, or does it simply remain an ordinary atmospheric air, in other words, is it possible for two birds to soar immediately in the wake of one another, extremely close in the wake of one another, or either directly underneath or above one another.

Mr. Alec. Ogilvie: I am afraid I am a poor hand at speaking, but I was connected with the Wrights when they were making some experiments with soaring this autumn, and all we did then, of course, was to glide practically in an upward current of air. The hill varied from about 1 in 6 at the top to nothing at the bottom of the hill, and we found we were able to soar fairly easily near the top of the hill. About half way down the hill it was difficult, and near the bottom it was fairly easy, but we were unable to get out at all in a horizontal line from the top of the hill, to any great extent, at any rate. The angle of the machine which we were using would glide at something like 1 in 9, and that 1 in 9 gliding angle—I should think it was going about 25 miles an hour, probably—would be a drop of 4½ feet per second. One could make a machine which would probably drop at about 3 feet per second. When I was coming home on the steamer we saw some very good upward soaring by gulls against the side of the ship. The wind was blowing straight across the ship and the gulls were soaring up and down it from one end of the ship to the other without any movement of their wings at all. I worked out the speed of the gulls about 27 miles per hour, and they made a turn in about 3 seconds or so, which would be about an 80 feet circle. Of course, that is under different conditions to the conditions which Dr. Hankin has seen birds soaring in India, because all my observations are in currents of air which are undoubtedly upward. Soaring under conditions of heat I have not seen, so I cannot say anything about it. I think they are the only actual facts I have to put before you.

Col. J. D. Fullerton, R.E.: Before discussing the theory suggested by Dr. Hankin, I may point out that it is, practically speaking, the same as one proposed by Mr. Albert Ross some fifteen years ago (see *Mean's Aeronautical Annual*, 1897, page 157). Writing in the *Marine News* he said: "The question is, do we now understand the properties of air? . . . It does not contradict the laws of thought to assume for the purposes of experiment that the constant action of the sunlight upon the air is such that when the air is agitated by the swift passage of the wing-bone a molecular action is caused in the air, which causes an expansion, and that expansion acting upon the rear feathers upward bent, gives a forward thrust." I do not suppose that Dr. Hankin has ever heard of Mr. Ross, but the extract is interesting as showing how two people, living many thousands of miles apart, sometimes hit upon the same idea, quite independently. As regards the theory itself, two things have to be proved, viz., that "ergaer" exists, and that the *air really was calm* at the time the soaring feats reported occurred. I do not propose to say much about "ergaer," as its existence or non-existence is clearly a matter for experiments, but I may here draw attention to the fact that hydrogen and chlorine explode when exposed to strong sunlight, and bearing this in mind, it is perhaps just as well not to condemn Dr. Hankin's theory too hastily.

Next, as regards the state of the air. The air may have been calm, but I am not quite convinced that it was so. The tests applied by Dr. Hankin do not seem to me quite satisfactory, as they were not delicate enough to detect small upward currents of from 2 ft. to 4 ft. per sec., at heights of from 100 ft. to 2,000 ft. above the level of the ground. Upward currents of this kind are amply sufficient to support the lightly loaded birds referred to, and consequently an examination of the tests is of great importance. Before discussing them in detail, I may point out that calm air is considered by meteorologists to be something of a "meteorological curiosity," and that the normal state of the air is one of motion, and very varied motion too. Velocities and directions, both vertical and horizontal, are constantly changing, and although calm air may of course exist, it does not seem likely that it would continue for the lengthy periods covered by Dr. Hankin's observations.

Next as regards the tests applied by Dr. Hankin. These were observations on the strength of the wind about ground level; noting the rise of smoke from chimneys;

watching feathers, &c., floating in the air; observing the formation of heat eddies and noting the effect of diminished sunshine on the soaring power of birds. No instrumental tests were made.

As regards the strength of the wind, the Beaufort scale states that a wind of five miles per hour (7½ ft. per sec.), is almost imperceptible to the sensations. I do not think it is possible, therefore, to judge the strength of currents of 2 ft. to 4 ft. per sec. in this manner, especially as the currents may have been stronger at soaring altitudes. The smoke tests from chimneys are also, I think, equally unreliable, as a vertical current of air could not be separated from the current due to the chimney gases. Floating feathers, &c., are also a very doubtful test; they must obviously have some weight and some definite falling velocity, and if they are supported in the air it seems natural to suppose that there is an equal upward velocity of the air supporting them. [N.B.—I have found by experiment that the small feathers from a pigeon's breast, weighing 1/25 grain, fall at the rate of 1¼ ft. per sec.] As regards the upward turn of the outer feathers of the wings, when a seed or feather was motionless in the air, this upward turn is due more to the weight of the bird than anything else. The reason why this peculiar arrangement exists is, as pointed out by Dr. Cousin, to ensure the bird's stability; the feathers of non-soaring birds do not turn up in this way. Deductions drawn from watching heat eddies seem to me of very doubtful value; delicate instrumental tests are essential if any reliable information is to be obtained. As regards the reduction of the amount of sunshine causing the birds to stop soaring, it is only fair to point out that clouds coming across the sky would reduce the strength of the small upward currents, and that alone would cause the birds to change their mode of flight.

But quite independently of the tests, I think the geographical and climatological conditions of Agra and Naini Tal are opposed to the likelihood of still air for any length of time. Agra lies almost in the centre of an enormous plain, sheltered on the north by mountains from 1,500 to 20,000 ft. high, in a country where the average yearly temperature is about 80° F. In January the mean minimum temperature is 42° in the early morning, while the mean maximum temperature is about 73° at 3 p.m. Similar conditions occur in July, but the daily mean range is from 77° to 93°. The mean horizontal wind velocity in January at 8 a.m. is from 2 to 5 miles per hour; the mean July wind at the same time is from 5 to 10 miles per hour. As regards Naini Tal, the village lies in a sort of cup; the level of the lake is 6,350 ft., and it is surrounded within a mile and often much less by hills from 1,000 ft. to 1,500 ft. higher. The temperature is not, of course, as high as that of Agra, but in January there is a diurnal range of 16°, while in July the rise is over 9°. It seems to me that both these places favour strongly the formation of small, steady, upward currents and I think it very doubtful whether the air there is ever calm for any long periods.

On the whole I am inclined to think that soaring is due to small, steady, upward currents. The bird apparently flaps himself up in the air until he comes to a stratum where there is a current having an upward velocity equal to his downward vertical gliding velocity, and he can then, by suitably inclining his wings, soar as long as the particular conditions last.

Mr. Mervyn O'Gorman: Owing to the lesser size of birds—even the largest—it is not proper to consider that their soaring is necessarily dependent on identically the same conditions as that of aeroplanes and gliders of man-lifting size. Small models may yet be made which will soar as birds do if they can be indued with certain organs which may for present purposes be called organs of sensitiveness.

In the two opening speeches made at the last Aëronautical Society's meeting on the 30th ult., Mr. Berriman and Dr. Hankin respectively considered two different aspects of soaring, and it is possible that the size of the aeroplane and bird respectively occupying the mind of each explained their divergence of view, if we add to the bird a supposed sensitiveness to small movements of air which will be suggested therein. It is agreed that the old conception of the air chiefly as a horizontally moving fluid, though useful to sailors and go-by-the-grounds, is too narrow; and we now admit the existence, if not the predominance, of vertical currents which are clearly the cause of the horizontal ones.

This conception, though broadly correct, is in detail insufficient, and we have been compelled to move forward yet another step by flying in aeroplanes, for we admit that the up, down, and cross movements are very local, as well as broad and general—one wing may receive air having one velocity and direction while the other wing receives another, during a time when the air as a whole is moving with a general trend from say west to east, or alternatively, while the air as a whole is so calm that no instrument or sensation of one detects a draught at or near the ground. In the case of flight near the ground we detect the causes of these *remous*—a pond, a patch of grass, houses, a chimney, a shed, hot sand, the shadow of a hill, &c.

Dr. Hankin, as the maker of most studious observations on birds, is best entitled to frame the theory which seems to him to meet and explain the observed facts; and I have no data to compare with his. He finds that while a feather falling away from a bird's body drops towards the ground, on a day which is only to be described as perfectly calm, the bird itself circles and soars without flapping. Moreover, he obtains a general conclusion, that soaring occurs best, though I gather not exclusively, in sunlight.

The effects of sun on the air, and on the ground which will in turn affect the air to be considered, and the possibility of the air being non-homogeneous, is to be particularly looked for. Air which has at some time risen from, or passed over, the ground, is liable to be irregularly moist, and just as the clouds have irregular shapes, so the invisible water vapour may be expected to occur in nodules, and streaks of varying form. The amount of invisible moisture is also liable to be irregular even within a moist patch, according to the amount of delay allowed it by the wind over wet grass, trees, rivers, sand, &c., and the temperature of those sources at which it gathered its moisture.

At any plane vertical section of the atmosphere which is thus blotchy with damper places there is irregular air movement for an important reason, even if the temperature of the entire section were uniform. Still, in an irregularly moist atmosphere of uniform temperature, we cannot have anything like such rapid movements as are to be expected when the same air is irregularly warm. Hence the link between sunshine and soaring would appear to be far stronger than the sentimental one based on the extra cheerfulness of the bird. The direct rays of the sun have an extraordinarily small effect in warming dry air, but what is important now is that we have suddenly become concerned with relative heat absorption of various parts of the air, and we find useful information in the ordinary facts of physics relating to this subject. To begin with, the majority of the sun's radiant energy is translated into dark heat on its reaching the earth, and much of this is radiated therefrom into space, to be intercepted by any available good absorbers. Experiments made by Faraday (who seems to have done everything) and others show that air which is only partially moist—like the breatheable air in a room (contains only 42 per cent. of the moisture required for saturation), absorbs radiant heat 72 times as much as dry air. It is unnecessary to say that this increase of absorptive power will very noticeably increase its temperature, thence its volume, and thence its rate of rising.

Let us only consider a small lump on a nodule of moist air suspended and isolated at a high level above the earth. It will not be uniformly warmed, and even if it were, experiments on superposing a layer of cold liquid on a layer of the same liquid hot, have shown that the hot liquid does not rise as a lump, like a cork, but in streaks, which curl as they rise. Accordingly we may safely anticipate for the rising nodule a curly, irregular surface, breaking into streaks of upwardly moving air, immersed in a sea of, according to the first supposition, perfectly still air, or, according to the second assumption, air which is only moving here and there vertically according as it was more moist than the surrounding matrix. In addition to this, there is the irregularity due to the various degrees of saturation existing in the nodule.

If we suppose a bird to be gliding in this irregular fluid, and to have such sensitiveness in his bill, head, or leading edge feathers as to detect the presence and shape of the waves about him, we have, in a manner which has been so frequently explained that it need not here be repeated, the necessary conditions of soaring.

While on this subject it is interesting to remember the existence of other causes for portions of air becoming more buoyant locally in the sunshine, whether or not the general atmosphere is calmer, or in spite of the general atmosphere having either a downward or upward trend. Thus there are interesting facts known about other volatile substances, and other gases. Ozone, for example, has 136 times the heat absorption of dry air. Perfume-laden air is given as having many times the heat absorption of air.

Thus patchouli, otto of roses, lavender, thyme, rosemary, cassia, are credited respectively with increasing the heat absorption by 31, 37, 60, 68, 74 and 109 times respectively. Aniseed, as an extreme case, has the remarkable quality of causing the absorption of 370 times the amount of heat of dry air. Mixtures probably behave differently, but I have no data. Enough has been said to suggest further and different causes for irregular local ascending currents. To each of these ascending currents there corresponds some downward movement and some horizontal movement cross connecting the up and down streams, and the whole moving system is doubtless a mesh of curly moving items of air. How the bird becomes conscious of these " curlies " can only be surmised ; he may smell the perfumes, and even the dampness ; he may be capable of feeling the change of acceleration of the air movements as he enters upon their outskirts, and so of anticipating whether the particular curly he is next about to enter upon is rapid or slow, large or small, helpful or useless ; or whether it is one that calls for increasing the angle of attack or decreasing it.

The chief matter of interest in the above suppositions is that, in accordance with Dr. Hankin's interesting observations, the " curlies " will be brought into being by the sun, partly by the direct heat, but mostly by the dark radiant heat after the conversion has been effected by falling upon the earth, or upon dust particles in the air. The occurrence is quite compatible with the most perfect apparent stillness of the atmosphere, and with the fact that a feather a few inches from a soaring bird might be travelling downwards in one side of a " curly " while the bird soared with the help of the top side, or the rising side.

The continuance of the phenomenon depends upon the air failing to become homogenous through mixing and diffusion in the span of time during which soaring lasts. There is one physical phenomenon which may assist in this also, namely, that the viscosity of air increases with the temperature (unlike what occurs in most liquids). This increase in the viscosity of the warmer air will tend to make it hold together a little more, *i.e.*, to become streaky instead of becoming rapidly diffused. Nevertheless, it is probable that such forces as these are very small if not negligible, and that " curlies " should cease to be after a lapse of some interval, the extent of which I have no means of forming even the roughest guess. The other theories of soaring are, as far as one can say, quite uninterfered with by this supposition which is in no way intended to be antagonistic to them, and the present suggestion would probably fail if the general body of air were near saturation.

Mr. Jose Weiss : From occasional extraordinary results from my glider I have come to the conclusion that the difference between gliding and soaring is only a matter of degree, not of principle. I feel that most people, through insufficient investigation, fail to realise how slight a gliding angle may be made, therefore they seek in far-fetched theories the explanation of results that are due only to very great efficiency. The misconception is not as to the fact that the bird obtains energy from the air, but as to the amount that it requires in order to maintain its soaring flight.

The glider and the soaring bird are both falling bodies following the paths of least resistance. A glider having an infinitely small drift, must follow a path very nearly horizontal once it has acquired its natural velocity. A bird approximates very closely to a theoretical glider of this kind, and the energy required to change gliding into soaring is so small that I see no difficulty in accounting for it on the theory of wind. The least undulation in the atmosphere would be quite sufficient, and it is not surprising that soaring birds abound in more tropical climates where the sun, which is the principal agency giving rise to suitable movements of the atmosphere, is so much in evidence. It seems to me that the phenomenon of soaring emphasises the necessity of investigating

in the minutest possible detail the causes of high efficiency in bird flight, such as the motion of the air in the vicinity of the wing, and the nature of the disturbance set up by the passage of the bird through the atmosphere.

It might be possible to verify the gliding angle of birds by borrowing a few species from the Zoo and letting them fly about in a large empty hall. This would eliminate wind, and I believe that careful observation would enable a fairly accurate estimate of the gliding angle to be obtained.

MR. G. F. MORT: The variation in the resistance of the atmosphere and the change between the temperature and the amount of moisture present. During several months past I have been running an aeroplane at a place protected from the wind. The resistance of the air varies very considerably at a given speed of the propeller. A certain propeller running at practically identical speed gave a taut of 192 ft. lbs. with the barometer 29·1, 202 ft. lbs. with the barometer 29·9 and 209 ft. lbs. with the barometer at 30·4. There were also temperature changes, which were not very important. This gives a variation of about 8 lbs. with constant speed. These results were taken from tests only a few days apart. It is hardly necessary to say that there is a very marked difference between summer and winter running. I have not formed a rule to cover all these variations, but it seems to me that all the results cannot be accounted for by simply changing the density, and I rather suspect that there is also a further change in the viscosity of the air due to various causes, and it is sufficient to make accurate investigations. One thing I would say is that the resistance of the air is very considerably less in the tropics than in our own country, and has some bearing on soaring flight. A bird in the air generally glides in a one-sided way. We cannot see the movement of the air: if we could I think we should probably be rather horrified at the extraordinary loss of energy in an aeroplane. It is just possible that that also may have something to do with the soaring of a bird. The aeroplane as at present conceived is a one-sided affair.

MR. V. E. JOHNSON: Dr. Hankin in his most interesting and instructive lecture given in this room a few days ago referred to the question of experiments made by models for the purpose of testing certain phenomenon observed in soaring flight. Referring to Dr. Hankin's theory of "ergaer," I think the first question that rises to my mind is whether its action is purely a mechanical one? And the second question which I think one is inclined to ask is why should these explosions of "ergaer" take place so evidently on the underside of the bird's wing, and not on the upper. Assuming that an inanimate wing of the correct shape, travelling at the proper speed exhibits this same fact, then it appears to me that possibly some valuable information might be obtained by automatically projecting a model glider a very great number of times, say several hundred in a building, first of all forming the experiment in sunshine and then when the sun was not shining, but when the meteorological conditions generally were as nearly coincident as possible. If this theory were proved and applied of course to inanimate as well as animate objects then I take it that the mean glide in the second case should be greater than in the first. I should very much like to know whether Dr. Hankin really thinks that the same thing would happen in the case of models as with birds, and if so, if he would give some indication of the kind of experiment he would like carried out in order to fully test the question in that way.

DR. HANKIN: In reference to the suggestion that "ergaer" may be connected with some electrical change, I may point out that "ergaer" is, as yet, merely the name of the, as yet, entirely unknown mechanism by means of which energy is stored in the air in potential form. The suggestion that "ergaer" is connected with electrical change will acquire value when evidence is brought forward that such a change can result in storing energy. Mr. O'Gorman's valuable contribution to the discussion suggests other ways in which energy may be stored. He has mentioned certain substances, such as ozone, which are or may be present in the air, and which have some power of storing the energy of heat rays. Supposing it could be shown that ozone in absorbing heat rays is changed into some unstable compound that can readily decompose and so give up its energy, something would have been discovered that might be identical with "ergaer." I merely put this forward as an illustration, not as a suggestion, as to the nature of "ergaer." As I

have previously stated, I have evidence, some of which I have published, that leads me to doubt whether the sun energy that becomes stored in " ergaer " is derived from the red end of the spectrum.

Colonel Fullerton's quotation of a singularly close anticipation of the conception of " ergaer " would acquire still more interest if he would give us sufficient of the context for it to be possible to judge how far the opinion of the author quoted was a lucky guess and how far it was based on facts of observation.

I have as yet brought forward no evidence of humidity on soaring flight. Sun soarability in the extraordinarily dry air of the hot weather in Agra, appears to be identical in its general characteristics with the sun soarability observed in the very damp air of the monsoon season. Similarly I know of no differences between the phenomena of soarability a few hundred metres above the earth in Agra and at a height of 2 miles above the earth's surface at or above Naini Tal. Soarability appears to vary with (1) the amount of available sun energy, (2) some unknown conditions that exist, especially in the neighbourhood of storms, and (3) temperature. This latter factor affects stability. In November, 1911, a " cold weather depression " coming from Persia and Beluchistan caused a sudden and lasting fall of temperature over northern India. During this period of low temperature I observed soaring flight under widely different conditions of cloud, sun, dryness, humidity and wind. Whatever the conditions stability appeared much greater than it had been previously at a higher temperature. On two occasions I saw " lift-gliding " without any sign of the dorso-ventral axis instability that I had hitherto thought was inseparable from that form of flight. Tail-jolting movements almost completely vanished. Apart from these observations, the most extreme cases of transverse axis instability, in my experience, have always been observed when the temperature was high.

In reference to what has been said of the possibility of there being movements in the air in apparent calm, I have no doubt that this is the case, but it would be very difficult to maintain that such movements involve sufficient energy to explain the phenomena of soaring flight. In the hot weather, and in complete absence of cloud, it generally happens that in the afternoon the air has a kind of opacity in that vultures at a height of 500 metres or more are very difficult to see. They seem to have a greyish blue colour in the background of blue sky. During the afternoon, within two or three minutes of the time at which soarability decreases at low levels this opacity rapidly decreases, so that vultures at great heights become easily visible. I have a great many entries in my notes to the following effect. " 4.0 low level flapping first seen, 4.3 distant visibility." That is to say, at the time at which cheels at low levels began to flap occasionally, vultures gliding at great heights came into view. But that the increase of transparency of the air is not caused by the low level decrease of soarability is proved by the fact that if there are a few clouds in the sky, even very thin high level clouds; the air is, I believe, generally, if not always, highly transparent during the whole day. It is possible, though not proved, that the opacity in question may be due to the presence of heat eddies which, as I have elsewhere shown, are not the cause of soarability.

The question has been asked why, if " ergaer " explodes on the under side of the wing lifting the bird, why does it not also explode on the upper side of the wing driving the bird downwards ? In the first place I may point out that the answer to this question does not bear directly on the question now before us, namely, whether the conception of " ergaer " is one that is true or probable. There are only two possibilities. Soarability is due either to kinetic or potential energy. If the kinetic energy theory is knocked to pieces, only the potential energy theory remains. I have, as I think, done more in that I have given reason for believing that energy is stored in the air. As to how this energy is stored, and as to how it can become available, we must look to future research for an answer. It is a fact that the air under the wing is more compressed than the air over the wing, and that explosive gases are more readily exploded when compressed. But whether or not this has a bearing on the question propounded must be a matter for future research.

A reference has been made to model aeroplanes. I should like to explain that I did not suggest that models should be used for the purpose of investigating " ergaer." I suggested that models might be used to test the various statements I have made as to the different methods of steering and other adjustments employed by birds.

The proceedings then terminated.

GENERAL MEETING

THE THIRD MEETING of the Forty-seventh Session of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, on Monday, March 4th, 1912. The chair was occupied by Colonel Sir Charles Watson, K.C.M.G., and there was a large attendance of members.

MILITARY AIRSHIPS

BY LIEUT. C. M. WATERLOW, R.E. (AIR BATTALION) A.F.AE.S.

I am particularly glad to have this opportunity of speaking on this subject because airships are rather under a cloud in this country at the present moment. Putting aside those who are working on this branch under the Government, there is scarcely anyone who seems to have a good word to say for them. The Press, both technical and general, either treat the subject as a waste of money or else " damn it with that faint praise " which is, if anything, worse than condemnation. The reason is not far to seek. Only eight airship pilot certificates have been issued by the Aero Club in this country, and of the holders all, save one, are or were in the Army, and are consequently debarred from defending in the public Press the attacks which have so often been brought to bear against this type of aircraft. The disparagers have, therefore, had it all their own way, but I hope to-night to put before you sufficient facts and evidence to show you that there is another side to the question, and that airships have a very valuable part to play both on the battlefield and in civil life. It has been rather generally assumed that it is a question of " *either* aeroplanes *or* airships " for military purposes. I would submit to you that *that* phraseology is wrong ; it should be " aeroplanes *and* airships." We may draw an analogy from the Navy : their ships may be divided into two rough classes : the first, containing battleships and the larger " Dreadnought " cruisers ; the second, containing smaller cruisers, destroyers, torpedo boats and so on.

The characteristics of the first class are :—

(*a*) Very high offensive and defensive powers, and
(*b*) Lack of speed.

The characteristics of the second class are :—

(*a*) Less offensive power and very much less defensive power, and
(*b*) Much greater speed.

These are, of course, merely rough generalisations and must not be pressed home too severely.

No one condemns the use of battleships because they will not steam at 35 knots per hour, or torpedo boats because they lack 13-in. guns ; and so I would ask you not to stigmatise an airship as useless because it cannot travel 60 miles per hour, any more than an aeroplane as of no value because it cannot rise vertically in the air. In my opinion each has its own definite role, and the neglect of the one would be as great a mistake as the neglect of the other. The Commander-in-Chief on a modern battlefield should be able to appreciate the values of not only the aeroplane and the airship, but of the man-lifting kite and the captive balloon.

In assessing the merits or otherwise of the airship we must naturally take into consideration the difficulties with which it has to contend. These I classify as the weather, excessive transport, and hostile aircraft. And I want to deal with each at some length and to show you that they have been considerably overrated.

The weather, of course, includes such things as contrary winds, wind on the beam when you wish to enter your airship shed, rain, fog, lightning, gusts, rising currents, air holes and pockets. The airship is inherently stable and so has nothing to fear from gusts, *remous*, etc. They cause pitching and tossing of a more or less severe nature and so lessen the speed to a certain extent, but they are not things to worry about, and I cannot imagine that they would ever cause a pilot to refuse to make a flight. Rain,

snow, hail, and mist, of course, very quickly increase the weight of the airship, and necessitate either the sacrifice of ballast or the use of swivelling propellers in a horizontal position. For this, among other reasons, no wise pilot should ever set forth without a liberal supply of ballast on board either in sand, water or petrol. Fog is very unpleasant, but its dangers may be lessened by going slowly and by descending from time to time to ascertain your position. Lightning is a vexed question: some people declare that you have nothing to fear from terrestrial electricity unless you are directly connected to the earth; others, with equal vehemence, assert the opposite. There is a rather natural disinclination to make experiments. I will, therefore, dismiss this point with the remark that thunderstorms are best avoided.

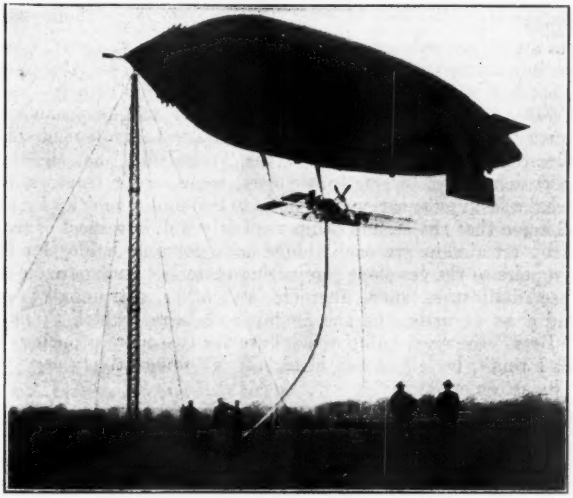
We then come to wind. An airship is perfectly safe as long as it is in the air; gusts, no matter how violent, have no ill-effect; and I do not think that even a vortex, that is, a current of air revolving in a spiral, would do any harm, though it would be uncommonly unpleasant. But when your airship is on the ground matters assume a very different aspect. A wind coming with uniform strength and not varying in direction more than a few degrees can be coped with more or less. It is merely a question of keeping the airship head on to the wind; but as everyone knows, in this country, this kind of wind is almost non-existent; personally I can only remember about half a dozen cases in the course of six years. But when you have, as you usually do, a wind varying very greatly both in intensity and direction, your airship becomes extremely difficult to handle; and should it by any chance get broadside on to the wind it means a disaster almost for certain.

Abroad airships are kept in colossal sheds of steel and iron, and unless the wind is more or less in the direction of the length of the shed the airship simply stops at home and does not go out. This is a very severe limitation. It means that with a wind of 10 miles per hour and upwards blowing across the mouth of the shed, the airship is useless. Attempts have been made to get over the difficulty in two ways. One is to make your whole shed revolve: an obviously expensive and impracticable proposal for an Army like ours whose function is over-sea work; the other is to build a wall or shelter along one side of the entrance to the shed so as to keep the wind off while the airship is actually moving through the doorway. This is a satisfactory but very cumbersome and clumsy method. This was the problem to which, in the early part of 1911, various people in the Aircraft Factory and in the Air Battalion began to give attention. In the summer of that year we received the account of how the Naval Airship had been moved out in the open by an attachment to its nose and with the gondolas resting on the smooth waters of a landlocked basin. The idea, which then took shape, was to construct a nose attachment which would stand the whole pull due to the pressure of wind on the airship, up to a strength of fifty or sixty miles per hour, and to attach this to a swivelling device on the top of a portable steel mast. Pressure of other work prevented anything being carried out until last autumn.

The mast is of steel lattice work and is made in three pieces; it rests on a block of oak, and four steel cables, which can be adjusted by blocks and tackle keep it upright. On the top is a vertical steel rod, round which revolves an object resembling an umbrella: this is made of wood covered with canvas. It is padded inside. In the centre of the umbrella there is a pulley block, and a heavy steel cable passes over this block and is secured at one end to a capstan at the foot of the mast. The other end is made fast to the nose of the balloon, from which point the strain is distributed by a system of ropes over the whole circumference of the envelope.

I do not propose to enter into any details of the device by which the end of the cable is connected to and disconnected from the nose of the balloon, beyond stating that it is so arranged that it can be made fast and released without bringing the airship down to the ground. As a matter of precaution we made up a dummy airship first out of an old sausage-shaped balloon, which had been out of use for many years. This was tried on three occasions, being left out all day. Although it flew with the utmost steadiness, on the ground it was remarkably difficult to handle owing to its poor shape and the absence of any vertical plane.

Profiting by the success of this model, it was decided to have a twenty-four hours' test of the "Beta." This was carried out on February 19th and 20th. The "Beta" was taken out at 9.45 a.m. on Monday the 19th, and remained up in the air for a short flight until about 10.15. At the end of the flight, the airship was brought down to 200 feet from the ground and about 50 yards to one side of the mast, and the tow rope dropped. This was seized by the detachment of men and the airship brought to the leeward side of the mast. It was then hauled down to within 80 feet from the ground, and the operation of mooring commenced. About five minutes later she was successfully moored. "Beta" remained moored from 10.30 a.m. on the 19th until 9.45 a.m. on the 20th. During that time a guard of three men looked after her, but for the most part only one was on duty at a time. A rope ladder was slung from the car to enable the crew to have access to her and also to allow the sentry to climb up every half-hour to see that the gas pressure was maintained. The engine was covered with a tarpaulin to keep the rain off. One man's weight in sand was hung on the end of the ladder so that, when the sentry ascended, instead of the airship tipping downwards to what might be an awkward angle, the bags touched the ground and equilibrium was maintained.



THE "BETA" ANCHORED

While the airship was moored the weather was extremely bad. Rain fell for most of the night. At five in the morning there was the best part of a gale of wind. During the time the airship was on the mast no gas whatever was put in, but pressure was maintained by putting air into the ballonnet by means of a 4-inch hose some 60 feet long, connected to a hand-blower on the ground. The point I want to drive home is that, although this airship was out in the open for twenty-four hours under very severe conditions of wind and weather, not a single cubic foot of gas was put into her during that period. The lesson we may learn from that is, that if the gas supply arrangements break down temporarily, it does not entail a disaster.

This leads me to a consideration of the second difficulty—referred to as *transport*. I mean by this the provision of the various stores with which an airship has to be supplied to keep it going.

In Continental armies this includes a portable shed for each airship, but, as I have just shown, we now have a mooring mast which can be carried in pieces on a single lorry,

and which, when reconstructed in a modified form, it will be possible to erect in a few hours with a dozen men ; and I think that a dozen men, possibly a few more, will be sufficient to carry out the mooring of any airship up to 200,000 cubic feet in volume. For comparison, I may add that the " Beta's " volume is 33,000, and that of the " Gamma " about 90,000. The average gas consumption daily of the " Beta " amounts to about 466 cubic feet, or under 2 per cent. of its volume.

This brings me to a consideration of osmose. Osmose, for the benefit of those who may have forgotten their physics, is the tendency of two gases separated by a membrane or porous partition to mix. In an airship the gases concerned are air and hydrogen ; so that when your hydrogen diffuses out, air diffuses in. The result of this is that, little by little, the hydrogen all escapes and its place is taken by air. The rate at which this takes place can be measured by the lift of your airship if taken on successive occasions. The rate depends on the material of which the envelope is composed. Goldbeaters' skin of which the " Beta " is made, is extraordinarily good. At the end of a month and a half, the lift of this airship had not appreciably decreased.

Returning for a moment to gas consumption, I think for a medium-sized ship—say one of 150,000 cubic feet—we may expect a loss of 2 per cent. per twenty-four hours ; that is, 3000 cubic feet. When compressed in a gas cylinder, or as we call them, tubes, this quantity will occupy six tubes each nine feet long and weighing about two cwt.

These tubes are at present carried in the field on horsed waggons, which are built to hold nine tubes, but motor transport, which is now coming in for this purpose, improves matters somewhat. A motor lorry will carry fourteen tubes or a few more at a pinch, that is, some 7000 cubic feet of gas. So that one lorry will keep one airship going for two days. We may, therefore, reckon that two lorries will serve one airship comfortably. When you realise that the new French unit, the " escadrille," has eight aeroplanes and that its transport consists of twelve motor cars, some motor tractors, a motor-driven repair waggon, and a fast motor car, which comes to two motor vehicles for each aeroplane ; I think you will agree that the airship compares fairly well in respect of transport. Other stores required by an airship are such things as a portable ladder for examination of the valves and repairs to the envelope ; repairing materials and spare cordage, both wire and hemp ; speed indicators, spare aneroids, and other instruments ; spare parts for the engine ; and a set of tools. Of the actual store boxes, which would be taken into the field with " Beta," the most bulky articles are the two spare propellers in their boxes, and they could, I think, be left at rail head. It is comparatively easy to fly with one propeller only in an emergency.

Summarising, therefore, I think we may say that an airship would require two motor lorries, one of which would carry the store boxes and the mooring mast, and the other fourteen gas tubes. The detachment of men would consist of from ten to twenty. An extra waggon would probably be required to carry the men's kits, camping gear and food, and the whole party could be accommodated on the three waggons. Officers might ride motor bicycles if not on board the airship. I do not think, therefore, that the transport of an airship can be considered unwieldy.

Before leaving the question of gas supply I want to say a word about hydrolite. Hydrolite is a chemical product composed of hydrogen and calcium, and is in fact a hydride of calcium. It is in many ways similar to calcium carbide in that, when put into water, it gives off gas, only with hydrolite the gas is hydrogen instead of acetylene. Coming to actual figures 62 lbs. of hydrolite, added to 62 lbs. of water give 1000 cubic feet of gas, so that if you carry hydrolite and water in an airship instead of ballast, you have on board a portable and convenient reserve of gas which can be kept for emergency. It has this additional advantage, namely, every 1000 cubic feet of gas made gives you an additional lift of 70 lbs., whilst you still have in your apparatus some 100 lbs. of calcium hydrate, which will serve for ballast. This substance might be used in a much more extended form, but for the fact that it costs £4 per 1000 cubic feet of gas obtained, and that rather puts it out of the question except as an emergency substance.

I now come to the most controversial part of my paper, that is, the question of the airship being attacked by an aeroplane.

Whenever I have heard this matter discussed, the airship's tactics have always been assumed to be that of just running away or seeking security at a greater height. In my opinion no airship should go on service without a weapon. This weapon might be a gun discharging a half-pound or three-quarters shrapnel shell with an automatic fuse-setter in the breech, the setting of the fuse being altered by the sights. The loading would be automatic and every fifth or sixth shot might be a tracer shell, that is, one that leaves a smoke trail behind, so that corrections in aim could be readily made. The aeroplane has, I think, three vital parts :—Its crew, its engine and its propeller. The latter is, for artillery purposes, a disc some six or eight feet in diameter, and it cannot be protected by armourplate. Any fragment of the shell striking would shiver it, and that would be the end of that aeroplane for the time being.

The aeroplane can attack in three ways :—

(1) By getting above the airship and dropping or firing something at it ;

(2) By firing a weapon from the same level ;

(3) By ramming, an exploit which may be considered only as a very final resort.

Guns or pistols, or other similar weapons, need a passenger to discharge them with efficiency, and I do not think the airship has much to fear from the aeroplanes with one man alone. Let us assume that the aeroplane intends to drop a bomb or even a large spanner into the envelope, and so rend it. On catching sight of the airship he will at once ascend and may quite likely have reached 400 or 500 feet higher before the look-out man on the top of the airship has spotted him. The airship will open fire as soon as he is within range, probably ranging itself broadside on to the aeroplane for the purpose, and keeping its propellers only moving just so quickly as to keep steerage way. When only some 600 yards separate them, full speed ahead is ordered, and the airship necessarily passes clear out of the track of the aeroplane, which cannot turn sufficiently quickly to get round above it. The same procedure may then be commenced again.

If, however, the aeroplane pilot has a rifle or other weapon on board (and it is, by the way, almost useless in the now popular propeller-in-front machines) he may decide to attack the airship at about the same height. The airship's tactics are, to stop dead and fire from its bow or stern, so as to offer the minimum target, and, when the aeroplane is some 1000 or less yards away, to turn the swivelling propellers into horizontal position and rise at a rate of 1000 feet a minute until it is well above the other. Variations of these tactics can be applied to other cases.

I hope I have said enough on this point to show that there are possibilities for the airship, and that it should be by no means the one-sided affair that some critics have made out. The strong points on the airship's side are three :—

(1) Greater manœuvring-power in every plane than the aeroplane ;

(2) Greater offensive power ;

(3) The power of standing still so as to get a perfect gun platform.

I do not propose to say much about airship versus airship except that superior marksmanship and navigating power on the part of the officer commanding are the determining factors. The correct tactics are, possibly, to run away. This has advantages :—

(1) You are firing from your stern, that is, *with* the wind, while he has the wind blowing into his gun-barrels ;

(2) You may lead him into positions where your own artillery on the ground and your own aeroplanes or airships can assist you ;

(3) Every shot you fire tends to help your vessel along, while everyone he fires retards him ;

(4) It is the only way in which you can be quite certain of not getting broadside on to him.

I have seen a large number of artillery practices at various kinds of aerial targets, both stationary and moving, during the last six years: the conclusions I have come to —which I put forward with the utmost diffidence—are that aircraft, travelling at 1000 feet elevation or under, run the most serious risk of being brought down by the ordinary field-guns and field howitzers that every army possesses. At 4000 to 5000 feet elevation and over, as long as the speed over the ground does not fall below 15 miles per hour, I think an airship may expect to be reasonably safe. Should the pilot find shells bursting unpleasantly close, I think his tactics should be to rise quickly another 1000 feet and then head off down wind at full speed, as this procedure will entail the maximum alteration to sights and fuse-settings. What the various forms of special guns mounted on "all-round" mountings may be able to do I cannot say; several types have been produced abroad, but none, to my knowledge, in England.

In leaving this question of war in the air, I should like to emphasise the point that the airship's role is defensive, not offensive, where hostile aeroplanes are concerned. But the airship *must* be able to protect itself; an unprotected airship could only be employed on night-work, and I hold that it has a far wider role than that. Incidentally this requirement quite puts out of court the small airship, that is, one under 100,000 cubic feet in volume. Nothing under that size could carry the necessary weapons and its ammunition, and at the same time have sufficient speed to withstand weather variations.

Critics of the airship sometimes say, "Suppose your engine breaks down. You will have to land in the open without help, and your airship may get broken up by the force of the wind." To guard against this we can do many things. Firstly, divide up your power into several units, so that if one breaks down the others can be used; airship engines have every chance, as a mechanic is told off specially to look after them, both on the ground and when in the air. If, however, the worst happens—all the engines have broken down, you are blowing out towards the sea, there is 30-mile wind on the ground, and there is not a single tree or hedge to shelter behind —a sufficiently unlikely, though possible, conglomeration of misfortunes—there is still one possibility by which your ship can be saved from destruction, and that is the ripping panel. We have now carried out some three or four experiments with the ripping panel of "Gamma." The first and second times it was used she split from end to end, since then it has been improved, and on the last occasion no damage whatever was done to the envelope.

The control of an airship is a subject of considerable interest, which I will review by mentioning some features of its evolution at Farnborough. On "Nulli Secundus" the balanced rudder was abandoned, only to be brought into favour again quite recently. The boat-shaped car for the crew was likewise abandoned, and has similarly been re-introduced. The need for a fixed horizontal or vertical plane soon became apparent. On dirigible No. 2 an elevator was situated in the bows, and was found to be much too powerful. There was a fixed horizontal plane aft. In 1909 the airship "Baby" was built, and subsequently converted into the "Beta," which is now three years old, and as good as ever, a record which has not been beaten in any other country. Originally it had three fins on the tail of the envelope, which were found to be too unwieldy for practical work. The uppermost vertical fin was first removed and replaced by a triangular sail, which was found to have a tendency to pocket the wind, and also to lack rigidity. At present there are three fixed planes, two horizontal and one vertical. There is also an unbalanced rudder and two small planes in the bow. "Gamma" was the next airship that was built; it had balancers on the tail, also elevating planes both front and rear, all of which have since been abolished. A box kite arrangement is fitted to the stern end of the frame, and is, if anything, too powerful. The most important point about the "Gamma" is the swivelling propellers, which are so arranged that they can be turned so as to push the airship up in the air or pull it down. It is an invaluable device, and I regard it as an essential feature on a military airship.

The crew of an airship should consist of the pilot, the steersman, the engineer, the wireless operator, the gunner, and last and most important, the observer. Seven is a good number for war purposes, of which three should be officers; the pilot, the observer

and one other. The pilot is the captain of the ship. He is responsible for the course, the elevation, and that the pressure in the envelope is maintained. The steersman maintains the direction by compass or by the instructions given him, and also the elevation as ordered. The engineer looks after his engines and all the mechanical equipment. The functions of the gunner and wireless operator are obvious; the spare man would probably be the look-out.

Wireless can be worked on board an airship successfully. While over Winchester last year the operator caught signals from ships in the Solent and also from the Eiffel Tower. Signals cannot be heard without stopping the engines, but this is not necessary for sending information. Code messages are absolutely necessary, at any rate at present, and some simple apparatus for coding and decoding a message would be very useful. Bomb-dropping is, in my opinion, a most practical and profitable method of employing an airship. Doing it by eye is out of the question, and some form of sight is essential.

Photography can be very useful, and we have had a number of excellent photos taken. Miniature parachutes have been devised by which exposed plates can be lowered to the ground. But it would be quite feasible to develop and print on board. The Balloon Company have at present an apparatus for doing this on the ground in daylight, and enlargements can be turned out in about half-an-hour.

The function of the Military Airship.

First and foremost, I put *reconnaissance*, and I look to the airship to supply detailed information on any point which is visible from above—things, such as the state of bridges, railways, damaged or in good repair, the actual numbers and composition of troops on the march or in camp, where the various supply depôts are situated, their extent and composition, &c. If the commanding officer requires *general* information of an advance, the locality of the enemy, and so on, he should send aeroplanes; if *speed* is essential, he should send aeroplanes; but if he wants complete and exact details, he should send an airship. And in connection with speed, it must be remembered that if the airship sends back her information by wireless, she may very well beat the aeroplane, which has to return with it. For whatever they may be able to do in the future, everyone must admit that aeroplanes cannot use wireless effectively yet.

Secondly in importance I put *offensive action*, this is, of course, bomb-dropping. It is being generally recognised now, that the bomb-dropping referred to in Tripoli did not come to much, and one reason for this was the insufficient quantity of bombs carried by any *one* aeroplane.* Several accounts state that the pilot was unable to see what damage he had caused. Bomb-dropping needs the whole attention of one man, he cannot steer an airship or pilot an aeroplane, he must give his whole attention to it, and, profiting by preliminary bad shots, have a second attempt at once. Demolitions come under this heading, and the most obvious things to attack are supply depôts, forage depôts, any mounted troops in camp or in close order, headquarters, wireless stations, vessels in harbour or dock, bridges and viaducts. The list can be continually extended, but the common point about these I have given is that they cannot run away or scatter.

I do not believe that observation of fire is practicable from an airship. After six years' practice at it from captive balloons and kites, I feel that the difficulty of transmitting your observations sufficiently quickly to be of use quite puts an airship out of the question. When doing this sort of thing in practice-camps from a kite or balloon a telephone is used, and the ground end of it is by the battery-commander's side. The confusion and trouble that come if the telephone breaks down—and it often does—leads me to believe that it is not *impossible* but quite *impracticable* for airships. Nightwork is a speciality for airships; they have the field to themselves. It should be possible to locate every bivouac fire in the enemy's line. By stopping the engines and listening it might be possible to hear the approach of reinforcements and supply columns, if a likely

* When this paper was written the news of the recent successful use of bombs from airships in Tripoli had not been received. The above paragraph refers to bomb-dropping from aeroplanes.

road is selected for watching. In this connection I look upon adequate silencing as of the first importance in an airship's engines.

In the description of *the ideal military airship* I am about to give, I wish it to be understood that the ideas expressed are my own and not authoritative.

Considering size: its volume would be between 150,000 and 200,000 cubic feet in volume; in comparison the "Lebaudy" airship was some 350,000 cubic feet and the "Clement Bayard" about 250,000. With hydrogen giving a lift of 70 lbs. per 1000 cubic feet, we then obtain a gross lift of 10,500 lbs. I propose to divide this up as follows; the weights given are only approximate, and have not been worked out in detail:—

Seven members of the crew at 150 lbs. each	1050 lbs.
Four engines of 100 HP at 4 lbs. per HP	1600 ,,
The car, frame, and envelope and gearing	4000 ,,
The gun	300 ,,
650 rounds of ¾ lb. ammunition	500 ,,
Wireless apparatus	500 ,,
Hydrolite and apparatus	500 ,,
Fuel and oil	1000 ,,
Ballast	1050 ,,

The *ballast* would be half water, and half fuel and oil. The *speed* attainable should be not less than 45 miles per hour; the Germans have already achieved this. I think with the HP I have mentioned it might be very much greater than this. The height attainable is limited by two things, the size of the ballonnets and the amount of ballast to spare. I think 8000 feet should be aimed at. The *crew* would consist of seven, but more could be taken if necessary. They would consist of one pilot, one steersman, one engineer, one wireless operator, one gunner, one observer, and one spare. The *engines* would be four, each of 100 HP, but arranged in units of 200 HP with a clutch, so that either or both pairs can be disconnected from the propellers at will. The clutches would be actuated as in a motor car by pedals beneath the feet of the steersman. But arrangements would be made for any engine to be disconnected from its fellow by a dog-clutch, so as to allow for breakdowns. A separate engine of one or two HP would be provided for the wireless, and it might also be used for electric lighting. The weapons would be of the character I have already described, and four sockets or firing positions would be provided, one in the bow, one in the stern, and one on each side. A bomb-dropping tube and sight would also be provided.

The airship should be constructed on the semi-rigid principle; that is, with a long girder close to the envelope, because by this means alone can head resistance be minimised. In determining speed, this and HP are the chief factors; it is instructive to note that the French constructors appear to have overlooked this point entirely. The car would be in the form of a narrow boat some 20 or 30 feet long and about four or five feet broad in the bow. Below the deck level would be the tanks for oil, petrol, and water ballast, and store boxes for ammunition, bombs, hydrolite, and tools. The propellers would be of the swivelling pattern. A vertical hole and rope ladder would give access to the top of the envelope, from which a look-out could be kept. The envelope would have two ripping panels and a valve on top as well as the usual automatic valve on the lower side. The controls would be within reach of the steersman; these include steering-wheel, arranged as in a motor, lever for elevating planes, clutches, engine throttles, and the hand-wheel for operating the swivelling of the propellers.

The whole subject is very big and very engrossing; unlike aeroplaning, it cannot be learnt in a month or so. A long apprenticeship is needed. For as long as everything goes well, the pilot's job on an airship is perfectly plain sailing, it is only when a breakdown occurs that great skill is needed.

Major Sir Alexander Bannerman, Bart., R.E., Mr. E. T. Willows, Captain Broke-Smith, R.E., Mr. Glyn, Mr. Percival Spencer, and Mr. Mervyn O'Gorman took part in the discussion that followed, and the proceedings terminated with a hearty vote of thanks to Colonel Sir Chas. Watson for presiding.

GENERAL MEETING

THE FIFTH MEETING of the Forty-seventh Session of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, on 26 March, 1912, at 9.15 p.m. The Chair was occupied by Mr. A. E. BERRIMAN.

EIFFEL'S RECENT RESEARCH WORK

BY F. HANDLEY PAGE, A.F.AE.S.

M. Eiffel has carried out a series of experiments on various planes, starting with a flat plane and afterwards with curved planes. I would call for particular attention to be directed to the shape of the pressure distribution curves of the planes. With double the curvature there is a great increase in pressure (Figs. 1 and 2). Pressure is due to the change of momentum corresponding to the angular deviation of the air stream.

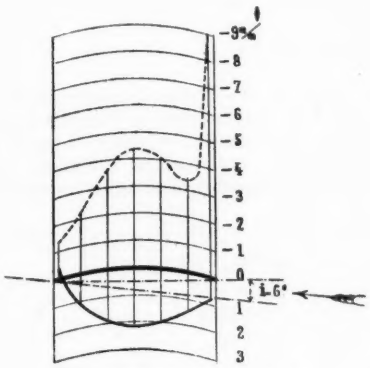


FIG. 1.—CURVATURE 1 IN 13·5

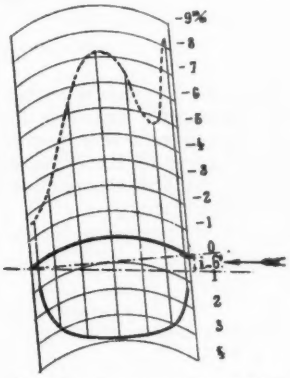


FIG. 2.—CURVATURE 1 IN 7·00

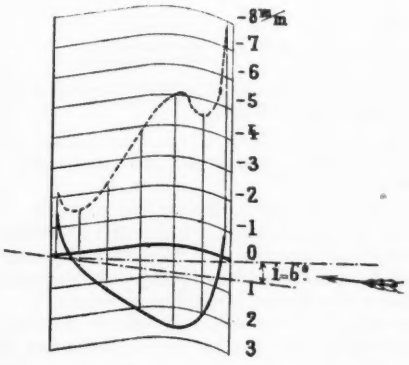


FIG. 3.—DIPPING FRONT EDGE WING-SECTION

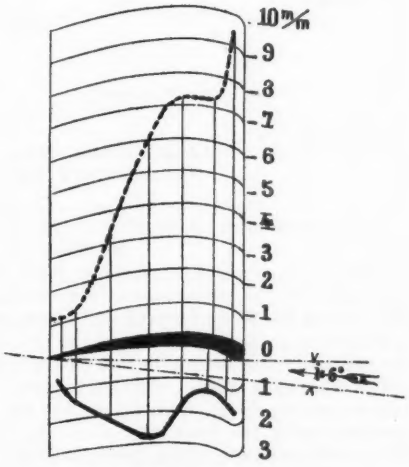


FIG. 4.—BIRD'S WING SECTION

After experimenting with ordinary curves, Eiffel tried planes more or less after the shape seen in an ordinary machine with a dipping front edge and a straight back inclined at 6° (Fig. 3). There was still the same rush of air over the front edge, causing an initial pressure which afterwards dropped. Eiffel has also taken what is understood to be a bird's wing, and the results show a big rise in pressure on the front edge (Fig. 4).

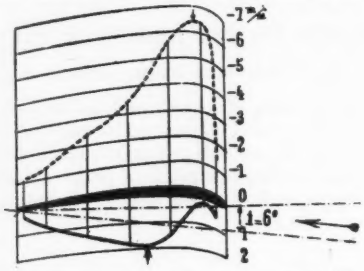


FIG. 5.—CROSS-CHANNEL TYPE, BLÉRIOT WING-SECTION

The result of pressure curves shown on a model of the Wright machine gives a pressure distribution curve which is not very good. A wing section of Blériot's Cross-Channel type shows a very deep front dipping edge. There is a nice shape of pressure distribution curve on the upper surface. The pressure is very small at the edges, and therefore there is little chance of eddies. There is also a diminution of pressure underneath the front edge (Fig. 5).

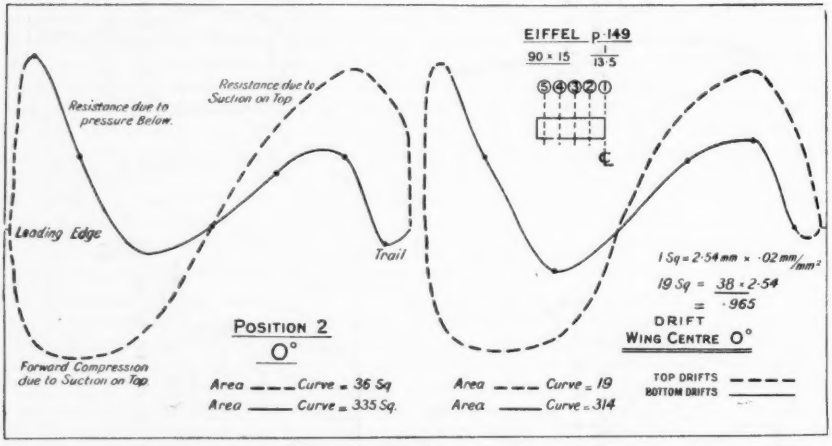


FIG. 6.—RESISTANCE ON 90 × 15 PLANE, AT 0°, AT CENTRE OF WING AND ALSO AT A POINT 150 MM. FROM CENTRE OF WING.

The more pressure obtained in front of a plane the more efficient the plane is. I have split up the resistances of one of the curved planes, and so found the resistance that helped the plane forward and the resistance that retarded it. The total resistance is, of course, the algebraical sum of the two, and if we could increase by any possible means the negative resistance on the fore part of the plane the efficiency would be increased.

A later-type Blériot shows a slightly less dipping front edge. It shows the same upper pressure distribution curve, but the underneath has not the small diminution of pressure under the front edge as in the other Blériot section. I have worked out the lift to drift ratios of every curve of every plane given in Eiffel's book,* and the plane

* "Le Resistance de l'Air et Aviation." Paris: Dunod and Pinat.

which has the best lift to drift ratio, viz., 1 in about 15, is that of Maurice Farman. Its maximum lift to drift ratio is obtained at a very small angle, and the lift at that point is very poor. The curve that is supposed to be in the shape of a bird's wing gives the worst lift to drift ratio of the whole lot.

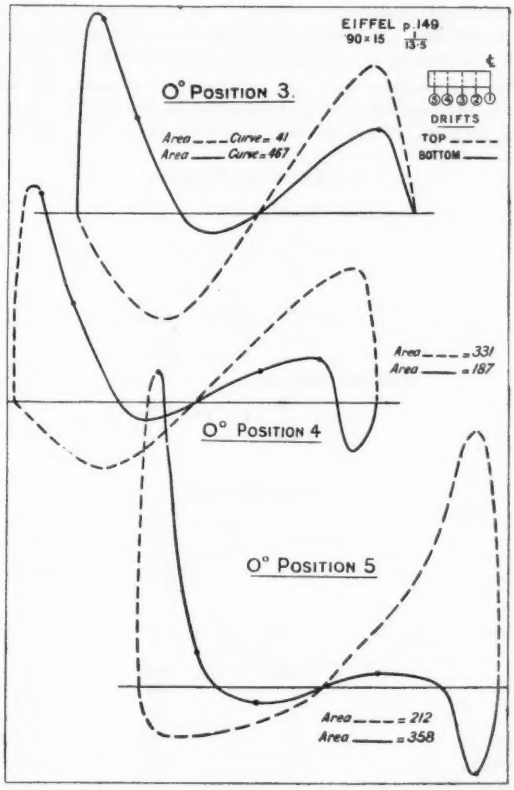


FIG. 7.—SAME AS FIG. 6, BUT RESISTANCES ARE TAKEN AT POINTS 300, 420, AND 445 MM. FROM CENTRE LINE, OR 530 AND 150 FROM WING TIP

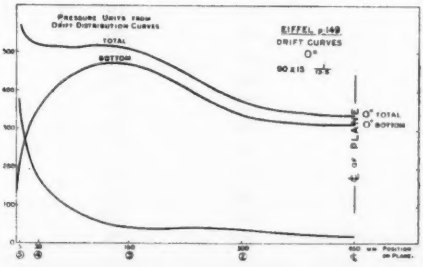


FIG. 8.—SUMMATION OF CURVES IN FIGS. 6 AND 7, SHOWING RESISTANCE AND LONG SPAN OF PLANE

It is not merely necessary to have a high lift to drift ratio at the angle at which one is going to work: it is best to choose a lift to drift curve which has a fairly wide range at a fairly high value. The Blériot Cross-Channel type has a very poor lift to drift ratio, being only 8, but it has a very good lift.

The best results as regards lift to drift has been obtained with single curvature planes, when the curvature is not more than 1 in 30 or 1 in 25. If the curvature is increased the lift to drift ratio seems to go down. Of course the lift is increased, but it is at the expense of resistance. It seems that if we curve the underneath side more than 1 in 25, the result will not be in the direction of increased efficiency.

With regard to the method used for calculating and analysing those planes, in a paper read before the Society last year (published in the April, 1911, issue of the JOURNAL), I fully explained a formula by which every one's experiments could be analysed.

ANALYSIS METHOD

Pressure (Total)—

$$= S \times \frac{W}{G} \times A \times V^2 \times \theta \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

$$= S \times \frac{W}{G} \times A \times V^2 \times \int_{\beta}^{\alpha} d\theta \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2$$

Pressure Density—

$$= S \times \frac{W}{G} \times C \times V^2 \times \frac{d\theta}{ds} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

SWEEP FACTORS

Wing.	Above.	Below.	Total.	From Pressure Curve.
Curve 1/27	·675	·405	1·08	2·35
Curve 1/13·5	·6	·29	·89	2·7
Curve 1/7	·58	·28	·79	3·87
Curve No. 5 (Eiffel)	·36	·183	·543	2·04
Curve No. 8 (Eiffel)	·53	·36	·89	2·88
Curve No. 9 (Eiffel) Bird				
wing 0°	·41	·48	·89	2·5
do. 2°	·45	·53	·98	
do. 4°	·49	·54	1·03	
do. 6°	·54	·46	1·00	2·5
do. 8°	·6	·49	1·09	
do. 10°	·61	·36	·97	
Wright	·59	·35	·94	2·0
Voisin	·54	·24	·78	2·5
M. Farman	·59	·15	·74	2·0
Blériot	·35	·19	·54	3·2
do.	·28	·17	·45	2·1
Breguet	·33	·28	·61	2·8
Nieuport	·49	·25	·74	
do.	·36	·43	·79	
do.	·47	·38	·85	
do.	·29	·25	·64	

If the results are taken for a flat plane and analysed, it is found that the sweep factor follows a definite empirical law in terms of the aspect ratio. The sweep factor equals ·35 times the aspect ratio, plus 85. Eiffel, in experimenting with his various planes, found a similar law for the dependence of sweep factor upon aspect ratio. One curious fact Eiffel has discovered is, that the slope of that curve was less than the empirical law which I deduced, and Eiffel's results are in every case less than the results obtained

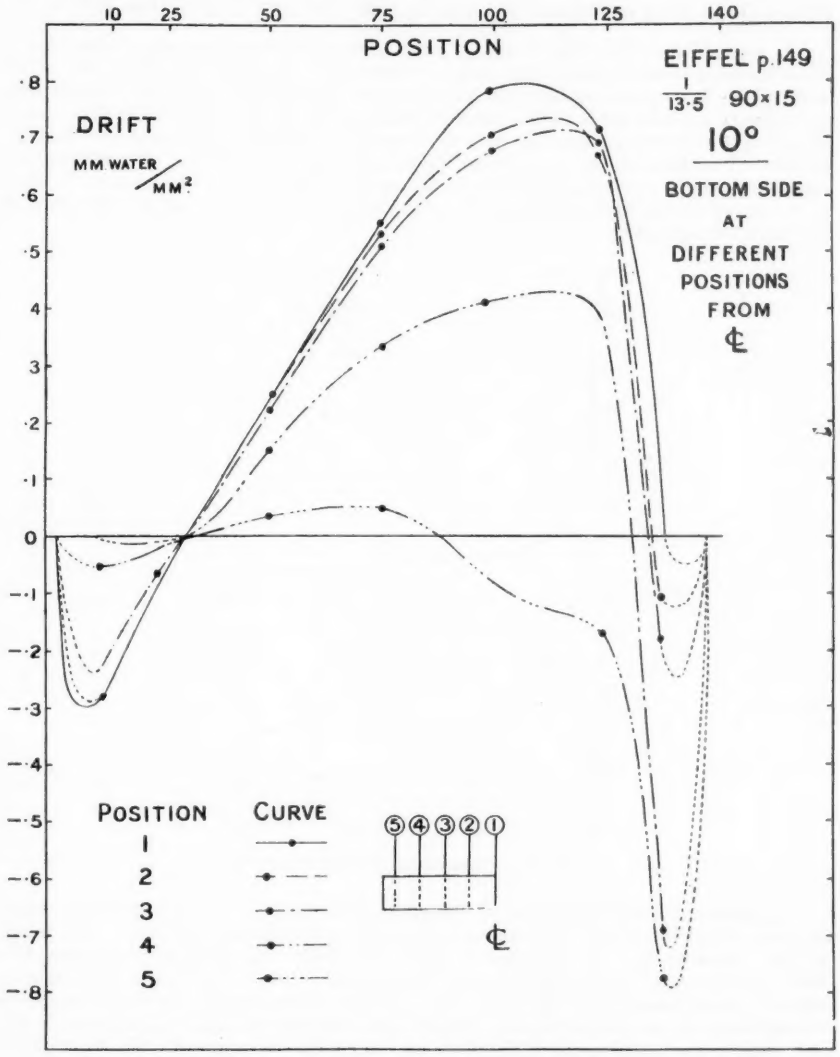


FIG. 9.—RESULTS 90 × 15 PLANE AT 10°. RESISTANCES ON BOTTOM OF PLANE.

by other experimenters which agree very well between themselves. It seems that in the case of Eiffel's experiments there is some cause which tends to decrease the values produced. A divergent current flowed through the tunnel in which Eiffel experimented,

and therefore a plane tested would have the air flowing out from the edges. The plane would thus have in effect a smaller aspect ratio. With a plane of, say, 6 to 1 aspect ratio the result obtained would be comparable with a plane in an ordinary wind tunnel, but with aspect ratio of, say, 3 to 1. Thus from the very nature of his experimental apparatus it might be expected that the results obtained by Eiffel would be less than those obtained by other experimenters, and that is the reason why Eiffel's formulæ gave smaller values than those I have given. The results of tests in Italy and in the National Physical Laboratory in England all agree, but they are much higher than Eiffel's.

It was quite evident that if one is going to fly with a biplane whose plane has a sweep factor of about 3, that is, the air being affected to a depth of three times the width of the plane, it is quite impossible to superpose an upper plane over a lower plane with the ordinary gap and get it to fly. If the results submitted are examined it can at once be said that everything connected with the formulæ is entirely wrong and not founded on fact. Two facts are involved, sweep factor and angle of incidence. By merely considering the angle of inclination of the plane as the angle to take, an injustice is done to the sweep factor, and credit is given to the sweep factor which ought to be given to the angle of inclination.

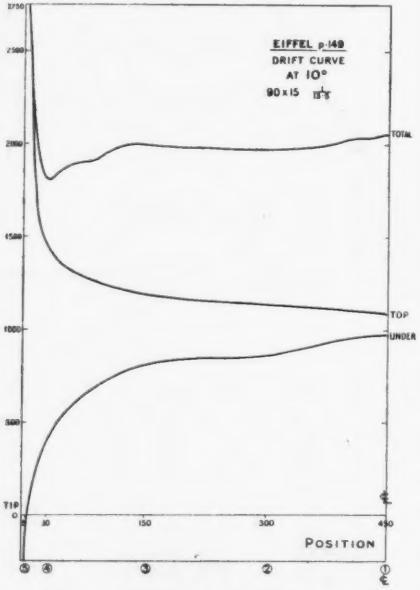


FIG. 11.—SUMMATION OF FIGS. 9 AND 10

A matter of great importance in designing an aeroplane is to find out that section of plane which combined with its area gives the most efficient machine. One has to obtain a little picture of what goes on above and below the plane, and one must take from experimental results the pressure distribution curve which gives the minimum amount of resistance with a maximum lift. Having obtained that, one has to obtain the sweep factors above and below, and then go back from the pressure of distribution curve and find out the actual curve of the plane for the conditions supposed. The curve which gives the minimum resistance is necessarily the most efficient curve to be employed.

With regard to the question of resistance of the plane. I have analysed the results of the 90 by 15 plane, whose results are given in Eiffel's book in great detail (Figs. 6 to 11). I have first taken pressure distribution curves at the centre of the plane and at

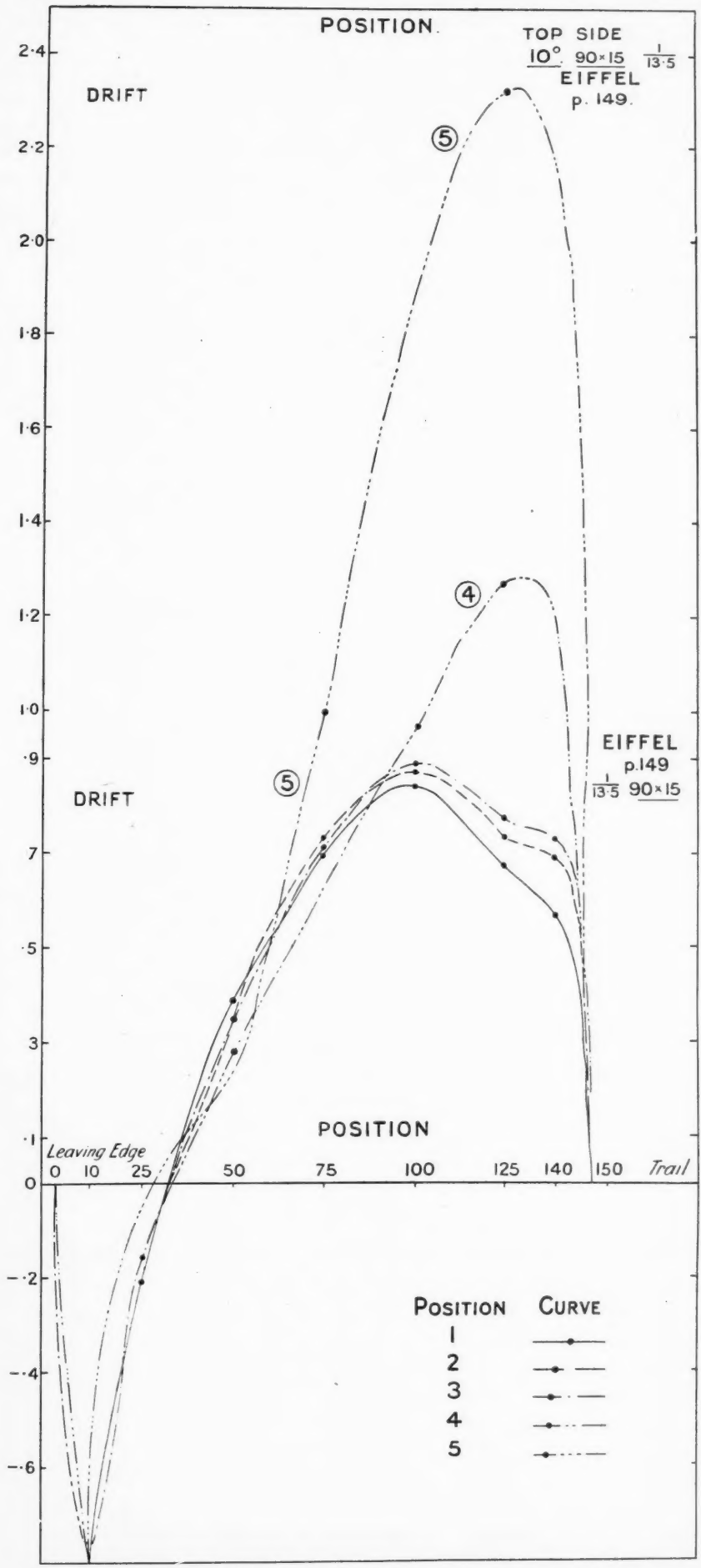


FIG. 10.—RESULTS ON 90×15 PLANE AT 10°. RESISTANCES ON TOP OF PLANE

various points along it, five places in all, and then have split up those pressure distribution curves into their two components, lift and drift. The resistance on the top of the plane at the tips is about double what it is in the centre of the plane, and this shows the enormous importance of giving the correct shape to the ends of the plane. It is far more important than the sectioning of the centre of the plane as regards resistance.

In Eiffel's book all the pressure distribution curves are plotted wrong.

One of the most interesting things is to calculate the actual value of the rising current at the front edge. In every case the current rises at a greater angle than the dipping front edge, and leaves at a less angle than the trailing edge.

In going through this data I think you will agree with me that although the method may be for a new machine somewhat laborious in the first instance, if you are armed with sufficient data, that is, if one has had sufficient experimental results you have all the figures and facts at hand with which you can plot the best section that you can possibly get for given conditions. There is one final result. When you have got the section it is very often probable that you will find that it is quite impossible to build it because the back part tapers to such a very fine extent that you are unable to get a back spar in, and therefore one has to discard it and make a section which has some approximation to the ideal one. But this certainly gives you a clue to what is going on, and also a method of getting what one may hope to be the best result.

Discussion

Colonel J. D. Fullerton said he would like to see a diagram with the whole of Eiffel's diagrams plotted out perpendicular to the surface. It was perfectly obvious that Eiffel's system was wrong—they should be perpendicular to the plane. He could not conceive how anybody thought of making them in the way Eiffel had. It made a very considerable difference if they calculated the lift and drift from the pressure when correctly drawn.

Mr. Mervyn O'Gorman said:—It had been suggested that Eiffel's results were exceedingly correct, because of their coincidence with others. Its margin of correctness was within about 10 to 15 per cent.—he might have been 10 per cent. out and not known it. In his wind tunnel Eiffel was measuring wind velocity—his results on the drifts would probably not be right within something like 10 per cent. Eiffel's measurements appeared to be more wrong than his results. The great thing which they had to accept and not depart from was the splendid thinking methods adopted by Eiffel as distinct from the experimental method. Eiffel went through an enormous amount of work, and for that purpose he designed the best possible tunnel. They were aware of the necessary slowness of operation with increased accuracy when the operator did not live in a tunnel. Eiffel's surfaces were much too large. His method obliged him to employ a 70 horse power fan which was expensive to run for many hours, and if the tunnel were bigger the expense would have been greater.

An investigation of the unexplained kink on curves would lead to appreciable valuable results. A curious point is the excessively high resistance of the plane which occurs at its extreme tips. Lanchester advocated the square ended plane because of its assisting the stability of the plane. If a gust should arise which impinges upon one wing more than the other its tendency is obviously to lift that wing and to throw the whole aeroplane round so that an added lift will be given to the wing which had not received the gust. The anomalous fact that a high head resistance located at the extreme tip of a wing should have a favourable effect on stability from any point of view is worth dwelling upon. It is like almost every other thing which one finds in the aeronautical hunt for stability, as soon as you have something advantageous from some one point of view then it is disadvantageous from some other point of view. Progress is very slow, and only time and careful attention to results can lead us forward. In this progress Mr. Handley Page's paper has helped us.

MR. T. W. K. CLARKE: If you had an aspect ratio of 6 to 1 at each end you could cut off a length equal to half the width, and so you get an aspect ratio of 5 to 1. You pointed out that in the Farman Machine there was a lift to drift ratio of 16 to 1; is that the best that has been done? I thought Rateau's curve gave 19 to 1.

MR. HANDLEY PAGE: About 16 or 17—the highest being about $1\frac{1}{2}$ degrees.

MR. B. MELVILL JONES: You make your calculations by imagining when you talk of sweep that there is a definite layer of air affected in the same manner throughout its section moving away from the plane, and then you take the radius apparently of that air, and you say in order to make it follow that path you must have a certain force on it from which you get your pressure of distribution. I think the method is correct in its way, but it is rather liable to produce a mis-conception in the minds of people that instead of really effecting a uniform thickness of air throughout you put on certain curvatures just at the filaments. The surfaces combined takes up the curvature of the plane, but the filaments (of the stream lines) further and further removed from the plane take up a little less and less curvature, and there is some place at which you can say a depth of air a foot thick being affected, but every layer from the surface of the plane to a distance away is moved in a less and less radius of curvature, and the result is that rather more is affected. The method that you use no doubt gives an idea of what happens, but it cannot be an accurate method, because what happens can only be arrived at by a complete analysis which is far too difficult to be attempted. You said that you could not reconcile certain ideas of sweep with the existing knowledge, and that gave me the impression that you were thinking of sweep as a definite layer of air which could be calculated direct from your results, and that there is nothing affecting you except this particular layer of air which is moving in a certain path. I have known several people who are continually thinking of this sweep, and getting an idea that it really is this definite layer of air moving *en bloc*, as it were, but that is not the case.

MR. A. E. BERRIMAN said that he was particularly fascinated with Eiffel's curves. They concerned the relationship in a sense between the static pressure that exists in the surface of the plane—an indication of what was going on dynamically in connection with the stratum that was being deflected. If the positive pressure beneath a plane remained constant, the rate of angular deflection of the equivalent stratum must remain constant; as the mass remained constant, the acceleration must remain constant, and therefore the rate of angular change remained constant. Since the acceleration is constant, the mean value over the whole of the chord is represented by a certain angle represented by the two tangents divided by the time.

MR. HANDLEY PAGE in reply said that whatever Eiffel might say about the wind tunnel being useful to take quick results, there was no doubt that as an accurate piece of apparatus from a aerodynamic point of view it was no good. He had not troubled to replot the curves. Although the different methods may agree remarkably well, Eiffel's results were wrong. It was a mere coincidence that they agreed. All machines made were not designed absolutely on theoretical lines. It was more reasonable to assume that the sweep remained constant over the whole plane rather than that the air followed the exact angle of the plane. He had never assumed that the air got to a certain point, beyond which it was not affected, but he had tried a method to obtain some practical figures so that he could draw a section of the plane which would give what he thought were the best results. The change of curvature being constant the pressure is constant. The distribution curve settled what curve your plane is going to be—the angle of incidence no longer entered into the question.

GENERAL MEETING

THE SIXTH MEETING of the Forty-seventh Session of the Aëronautical Society of Great Britain was held at the Royal Society of Arts, Adelphi, on April 15, 1912, at 8.30 p.m. Professor J. Perry, F.R.S., presided in the chair.

AUTO-MECHANICAL STABILITY

BY T. W. K. CLARKE, B.A., A.M.I.C.E., A.F.Ae.S.

The various methods of stabilising an aeroplane, I divide into three classes :—

(1) Inherent.

(2) Mechanical or Personal.

(3) Auto-mechanical.

In (1) the stabilising is effected by an appropriate disposition of the surfaces relative to one another (*i.e.*, dihedral angles ; leading planes set at positive angle of incidence to those behind, &c.), so that the machine tends to preserve a constant " attitude " to the relative wind. If, then, this latter is gusty the machine's attitude relative to the ground is continually changing, and therein lies the great disadvantage and danger of this system, especially when the machine is near the ground.

In (2) stabilising is effected, independent of any natural or inherent tendencies, by ailerons, &c., worked by hand.

In (3) the same as (2), except that the ailerons, &c., are worked by an automatic mechanism instead of by hand.

Now at first sight (1) would seem the best method of stabilising, but we have seen that the very same causes which automatically give an even line of flight without effort on the part of the pilot in calm weather, will, in rough weather—when an even keel is most wanted—give an irregular and dangerous flight path. For this reason stabilising by this method is generally carried out to only a small degree, so that any oscillations set up are slow and long.

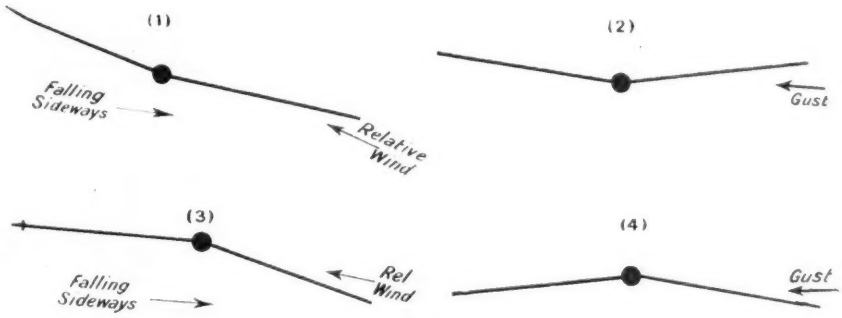


FIG. 1

As a further example, consider a machine with the ordinary positive dihedral angle, if it should become tilted, as in (1). It will commence to fall sideways as shown, and a relative side wind will result (the actual wind is not, of course, completely sideways, but has a small component in the direction shown). This, it is easy to see, tends to restore it to the horizontal position. If, on the other hand, it is flying, say, on a level keel, as in (2), a side gust will tip it up and cause it to slip sideways, *i.e.*, it disturbs its equilibrium. But, if the surfaces were bent well downwards at their tips, as in (3) and

(4), instead of upwards, any such motion or gust as above would have the reverse effect, *i.e.*, would drive downwards that side from which the gust comes.

Now between these extremes there is a mean form for the surface (it will be one having a small transverse curvature downwards), which, for side gusts or motions, within certain limits will have approximately no effect in tilting the machine one way or the other. "Within certain limits" I say advisedly, for although it may be possible to build the surfaces so that they are "neutral to gusts" at ordinary small angles of incidence, yet, at present, it would seem hardly possible to construct efficient surfaces that would be "neutral" to any gusts at whatever incidence the gusts may arrive, or if they struck the machine at certain points only.

I should here mention another general method and probably a better one than the dihedral angle method of obtaining inherent stability, viz., the flexibility method, in which certain portions, *e.g.*, the tail, are connected elastically with the main part, so that with an increase in the speed of the relative wind the machine tends to flatten out. The theory of such an arrangement was very simply put by Mr. Walkden in a recent copy of *Aeronautics*.

But, at any rate, the safest machine and easiest one to control would appear to be that in which the equilibrium is "neutral" and is then effectively controlled by a pilot, or, better still, by some automatic mechanism, so long as this latter is effective and not liable to failure, and is capable of being operated in conjunction with a certain amount of hand control for purpose of starting, landing, and small additional adjustments during flight.

Such automatic controls I would divide into two classes, *direct and indirect*, the *first* employing those devices which are operated by reason of the machine having got into a bad position. For example, when the machine is tilted forward 10° or more, its attitude may then cause a pendulum to operate the elevator so as to cause a righting couple on the machine, until the tilt is reduced to less than 10°.

The *second*, or indirect class, is that formed from those which employ devices which are operated by the same causes as those which produce the dangerous attitude in the machine; for instance, the device might be operated by gusts either longitudinal or transverse, or, as in the particular apparatus about to be described, by the actual motion of the machine, which in time would produce a dangerous attitude (either longitudinal or transverse).

In any automatic mechanism of the first class there must be some device which has a "directive effort," *i.e.*, some part which tends to assume or preserve a direction more or less constant with regard to external objects and is independent of the attitude or direction of flight of the machine.

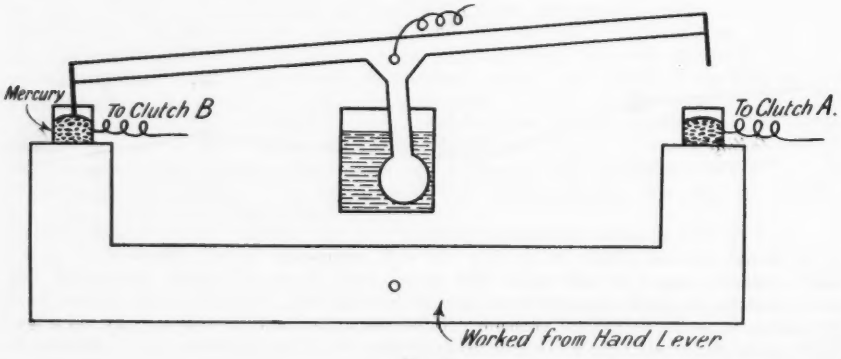


FIG. 2

Now the only practical devices that I can think of which have a directive effort are the pendulum, the magnetic compass, which includes the level and other gravity operated devices, and the gyroscope. In the second the effort is so small, and so easily deranged that it is of no practical use, and may, therefore, be neglected.

The first would obviously appear to be a useful device to employ, but there are objections, viz. :—

The periodic swinging of the pendulum is liable, under certain conditions which are quite likely to occur, to set up dangerous oscillations in the machine.

This disadvantage can be overcome to a certain degree by damping the oscillations, and so making it dead beat. This is usually done by making the bob or a vane attached to it dip into thick oil. It has also, but to a very limited extent, been accomplished in some instances by using a gyroscope for the bob of the pendulum. There are other methods, and one in particular with which I am acquainted, which so far as I know, has not been employed in this connection. This is by using the dragging effect on a copper disc revolving between the poles of a magnet, the copper disc is pierced so as to throw its c.g. out of centre and form a pendulum.

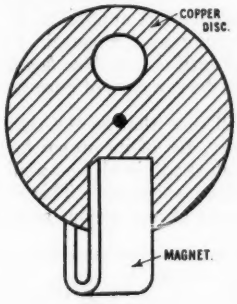


FIG. 3

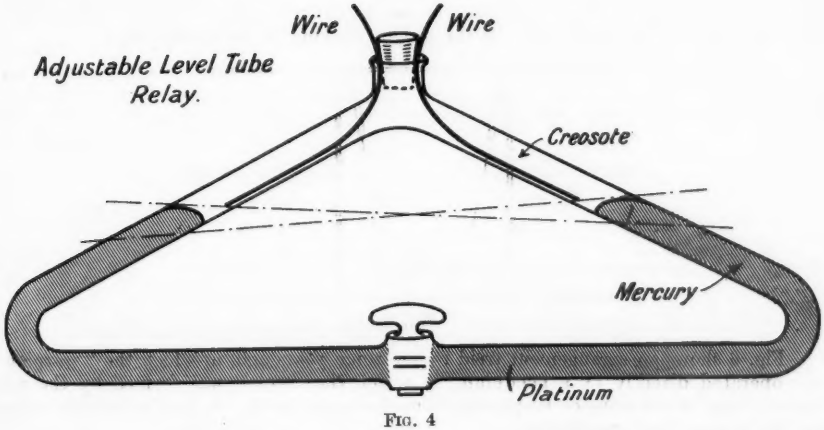


FIG. 4

Fig. 3 shows also an apparatus of this species made by me; the tap is for varying the degree of damping.

There is an effect when using a pendulum apparatus which is apt to be overlooked. It is best exemplified by Fig. 5.

If we have a trolley running without friction or resistance down a hill as shown, and having a pendulum suspended from it, then the pendulum will continue to hang perpendicular to the trolley as it is shown further down the slope, and the same holds even if there is resistance due say to air, &c., so long as the resistance on each (the bob and the trolley) is proportional to its mass.

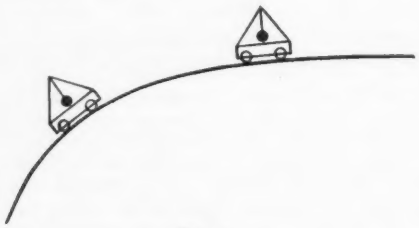


FIG. 5

But there is one other big disadvantage, common to most gravity operated devices, in this kind of control: the corrective effort applied to the machine only comes into operation when the pendulum has swung over a certain amount relative to the machine; that is, *the control is not actuated until the machine has already assumed an undesirable tilt.* In this respect, this method is, in fact, a case of cure rather than prevention.

If now we go further we will note that as the righting couple is put into operation when the machine has canted, say, 10° of its proper level position, and by this time the motion set up by the corrective effort will in all probability swing it well beyond its proper position, *i.e.*, the correction will be overdone. This difficulty could doubtless be overcome, but the writer does not know of any serious attempt in this direction.

There is a third disadvantage in some suggested systems of this class, namely, in those that work the controlling surfaces or weights, &c., as the case may be, directly, and without employing any sort of relay, and that is, that the pendulum or other accelerometer must necessarily have very considerable weight in order that the resistance due to the friction of the connections and the unbalanced forces on the controlling surfaces may be small compared to the resistances overcome in doing the useful part of the motion, and may have no material effect on the proper working of the apparatus.

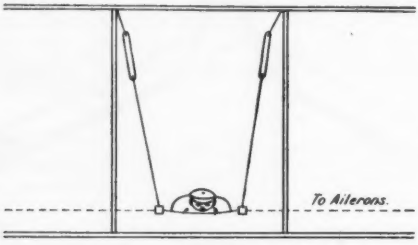


FIG. 6

Fig. 6 shows an arrangement tried by me some years ago, in which the "warping" was operated directly by a pendulum, in which the "bob" was formed by the pilot himself lying in a hammock suspended from the upper spars. It was practically useless for the reason just mentioned.

Next, if, instead of (as we do when using the pendulum,) employing gravity to give the directive effort we use the property of a *free* gyroscope of trying to maintain its axis in a permanent direction in space, we obtain a device similar to that when we use a pendulum, but with the advantage that the gyroscope is not subject to oscillations; moreover, the "direction" is quite arbitrary and not confined to the vertical, but, on

the other hand, should the direction of that axis become disturbed (and it is always subject to " creep "), it will not return to its original position unless it be subjected to some restraint tending to bring it back again. For instance, light springs may connect one end of the axes to some fixed point on the machine or the gyroscope may be attached to a pendulum, as mentioned above.

In the first case, the control would tend to steady the flight path, while in the second it would also give it " permanence " of direction. But such control would still be subject to the great disadvantage as to lag.

The remarks made above respecting direct controlling pendulums and their necessary great weight apply with equal, or even greater, force to the " free " gyroscope.

Consider now the other, or what I have termed the " indirect " class of automatic controls, which are operated by the same causes as those which tend to make the machine assume a dangerous attitude. Among these, gusts (positive or negative) are obviously very powerful, and there are many devices which employ subsidiary surfaces, which control by reason of the varying pressure against them ; in some devices there is a plane

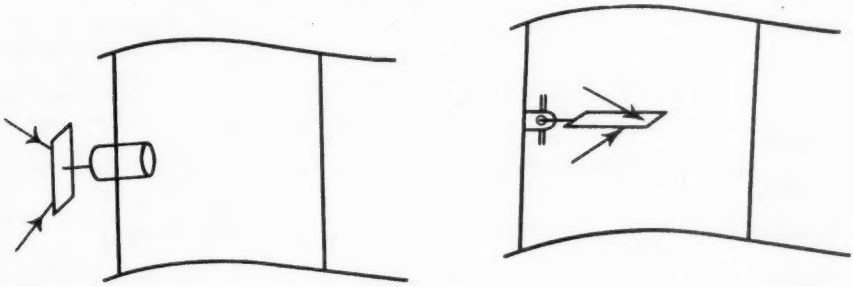


FIG. 7

surface set normal to the direction of motion, but in this case, although it is affected by an end on or upward gust, another similar one with a downward trend, would have the same effect on the control, but totally different effect on the machine. A much better arrangement is to pivot the surface so that it lies in a normal line of flow of the air stream (an example of this is the Wrights' apparatus). But even with the best of such arrangements it is the case of the control of a large machine by the conditions which exist at one particular spot, and which may be quite different at other points of the machine. For instance, it will sometimes occur that there is an upward gust on one of the wing tips while there is a downward gust acting on the central portion, where the apparatus would be placed.

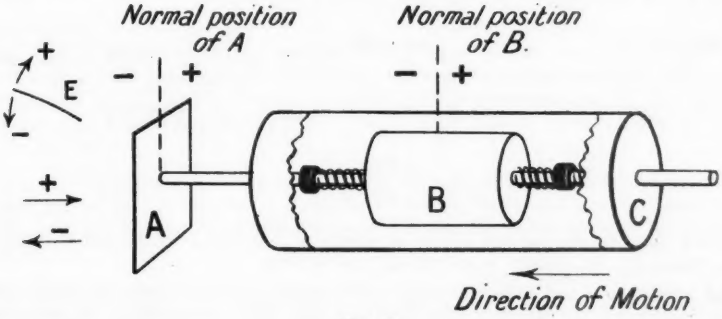


FIG. 8

The pressure plate method and the inertia method are sometimes combined. Such is the case with the Doutre apparatus. Fig. 8 is a diagrammatic sketch of the apparatus.

The casing C is fixed to the machine.

B is a heavy mass sliding on a rod to which the pressure plate A is attached.

The mechanism (air-valves, &c.) is such that when A and B close together the elevator E is turned + (*i.e.* up). [Or it may be when B moves into + region relative to C. On this point I am not certain.]

The motion of the elevator is proportional to that of B.

With an obvious symbolism we can then form the following table of cause and effect:

Cause.	Machine's consequent velocity.	A moves.	B moves.	E turns.	Effect.
+ gust ..	>	+	—	—	Instead of pitching up under the gust, the machine keeps on an even keel
— gust or + gust tailing off	Just the opposite.				
Propeller stop	>	0 to —	—	— to 0	Dips
Dipping ..	<	+	0 to —	—	Pitches further
Steady planing	=	+	— and remains steady	— and remains steady	Turns down a certain amount and stays there if nicely adjusted

Among the indirect methods there is a recent American patent (for transverse stability) which is of interest.

In this the difference in the resultant of the top and bottom pressure loadings of the two wing tips is made, by means of a Venturi tube inserted in each, to work a relay by which the ailerons are operated, as shown in Fig. 8A.

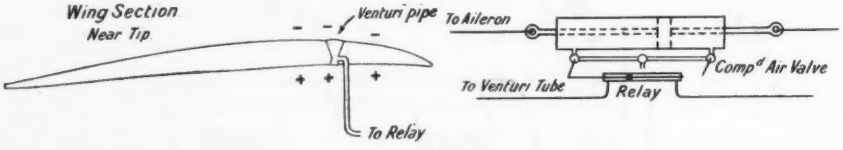


FIG. 8A

Now I have shown various causes or effects of instability which can be used more or less efficiently to operate a stabilising mechanism.

But there is *one* and only one cause which *must*, if not stopped, inevitably produce disaster, and that is the actual motion itself of either rolling or pitching. If, for example, a machine continues to roll over one way it will in a short time fall sideways, and be destroyed. Here then seems to be the radical and straightforward method of control, viz., to use some device which is operated by the actual motion of rolling or pitching.

The following is a description of an apparatus using such a device, and for the sake of clearness we will imagine it applied to the elimination of the transverse rolling motion.

The device employed is a gyroscope with two degrees of freedom, *i.e.*, the frame carrying the revolving heavy rim is mounted so as to be moveable about an axis at right angles to the axis of the revolving rim. If, now, this whole device be constrained to revolve about some line at right angles to each of the two axes above mentioned, the frame of the gyroscope will turn on its axis, such motion is called precession.

To get a better idea of what causes this motion, and of the relative forces involved, it will be convenient to lightly run over the theory of the gyroscope by the aid of a diagram.

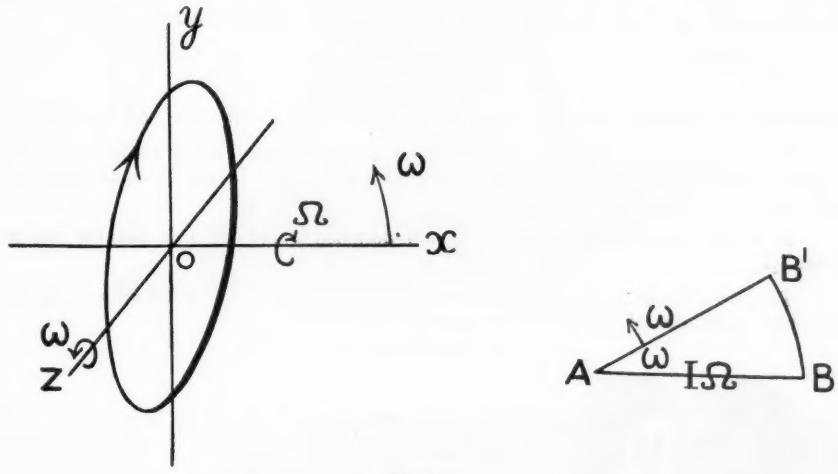


FIG. 9

If a heavy wheel whose moment of Inertia is I, is revolving Ω about its axis Ox, and is then turned with vel. ω about Oz there will be produced a Torque$=I.\Omega.\ \omega$ (*in abs. units*) about Oy.

[If Precessional motion about Oz is unhindered there will also be a resisting torque about $Oz = \frac{I^2}{i}\Omega^2\,\omega.t.$]

Suppose Ox, Oy, Oz are three axes at right angles to one another, and let Ox be the axis of a gyroscope revolving with angular velocity Ω as shown, then if we twist the gyroscope about the axis Oz with angular velocity ω, the gyroscope will exert a torque $I\Omega\omega$ (where I is the moment of inertia of the gyroscope rim about Ox), about the axis Oy and in the direction which tends to make the rotational sense of Ω the same as that of ω.

Proof by Hodograph or vector method. Draw AB$=I\Omega$ parallel to the axis of the gyroscope, *i.e.*, parallel to Ox, then AB represents the angular momentum of the gyroscope about Ox, and the velocity of B will represent the angular acceleration of momentum of the gyroscope.

But AB parallel to Ox has an angular vel. ω about an axis perp. to the paper, therefore B has a velocity $=AB.\omega = I\Omega\omega$ parallel to Oy. Therefore the gyroscope has an angular acceleration of momentum $=I\Omega\omega$ about Oz, and therefore a torque of this amount has to be applied to produce this, failing which it will "precess" or turn in the opposite direction, and this direction will be seen to be as above stated.

For the purposes of the apparatus we are considering, this is as much as we need to know, but for purposes of a quantitative study of the effect of heavy gyroscopes mounted as above when used as a species of anchor for a machine, we need to go somewhat further.

As shown, the motion ω about Oz causes a torque—$I\Omega\omega$ about Oy; this will produce a torque about Oz, resisting the motion ω.

Quantitatively, if we denote by ϕ and $\dot{\phi}$ the angular velocity and acceleration about Oy, and if i is the moment of inertia of the gyroscope and frame about Oy,

then $i\dot{\phi} = I\ \Omega\ \omega$.

and R the moment of the torque about Oz resisting ω is given by $R = I\Omega\phi$ if ω is constant, as well as Ω, the first equation gives us $i\phi = I\Omega\omega\ t$; where t is time in seconds during which the motion has lasted, and the second equation becomes

$$R = \frac{I^2\Omega^2\omega t}{i}$$

R being the resisting torque, i = moment of inertia about axis of y and t = time.

From this we see that *this gyroscopic resistance does not exist until precession has started*, and increases with the time, it varies as the square of the speed of spin, and directly as the speed of tilt.

It is also easy to show that in a well-designed gyroscope i is slightly greater than $\frac{1}{2}I$.

To return to our particular apparatus, this, the design of Mr. Johnson and myself, consists essentially of the following main portions :—

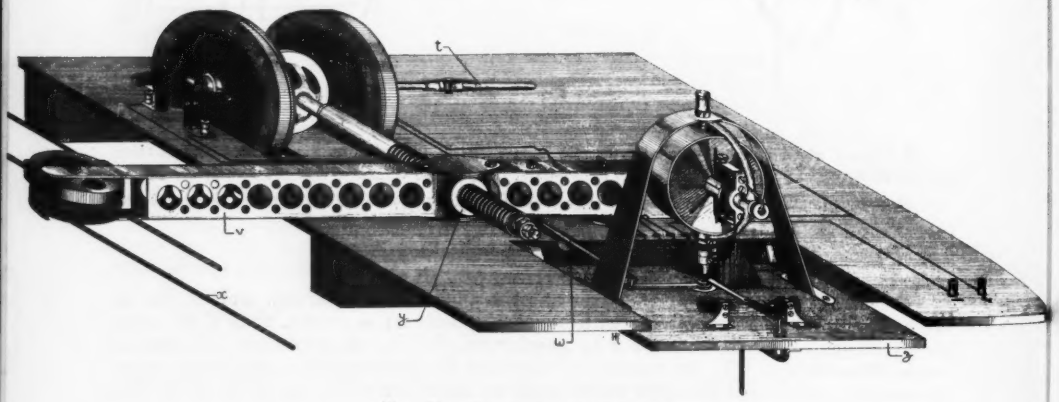


FIG. 10.—GENERAL ARRANGEMENT

(1) A gyroscope of quite small dimensions, and electrically driven.

(2) A magnetic clutch, controlled by the gyroscopic precession of (1), and by which power from the engine is transferred in one or other direction to

(3) A lever arm which works the running cable to the ailerons.

(4) A link piece and floating lever by which the whole motion of arm (3) is made to be proportional to the amount of precessional movement of the gyroscope.

(5) A ten volt battery for driving the gyroscope, and working the clutch.

There is also an independent hand control system. The effect of the two systems, hand and automatic, being cumulative; that is, if a certain movement of the automatic control rotates the ailerons 2°, and a movement of the hand control would rotate the same ailerons 3°, then when both are worked at the same time the ailerons would be moved 5°.

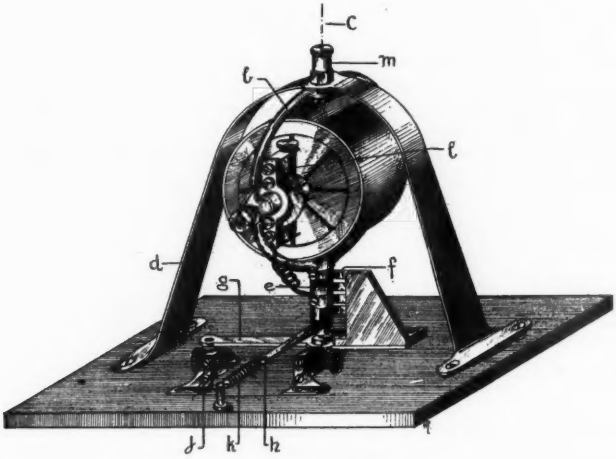


FIG. 11.—GYROSCOPE

The general arrangement can be seen from the illustrations, and to more clearly illustrate some of the points a diagrammatic sketch has also been drawn. (Fig. 13).

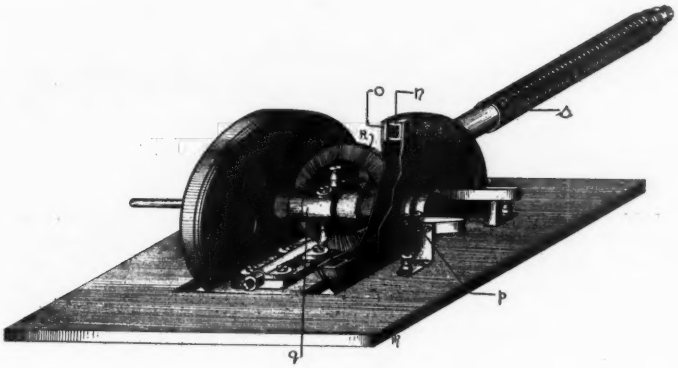


FIG. 12.—CLUTCH

The gyroscope is about 4 inches in diameter, and is mounted with its axis horizontal, and normally transverse to the direction of flight, while the frame *b* carrying it is about a vertical axis *c*, but pivoted motion about this axis is limited by means of an adjustable spring stop *j* to about 30°—40° on either side of its central position.

The gyroscope itself consists of an electric motor in which the outer portion, which usually forms the field magnet, revolves, while the inner portion is fixed.

Any transverse rolling of the machine at once causes the gyroscope to "precess," that is, the frame b turns on its axis either to the right or to the left, according to the direction of tilt, and thereby brings ring e_2 or e_4 into contact with f_2 or f_4, causing either one or other of the "pot" magnets n_1 or n_2 to become excited. This one then draws its armature ring o_1 or o_2 into contact with it, and the friction between the two carries them round together. The magnets n_1 n_2 are rigidly connected to the shaft t which is continually driven in one direction by a small gear wheel driven off the engine, while the armature rings o_1 o_2 can run freely (so long as neither n_1 nor n_2 is excited) in either direction, while they are at the same time continually in gear with the bevel wheel r which drives the screw s, thereby moving the lever (to which are attached the control wires), to the right or left, causing the right or left aileron to be depressed, and at once counteracting the tilting which has started.

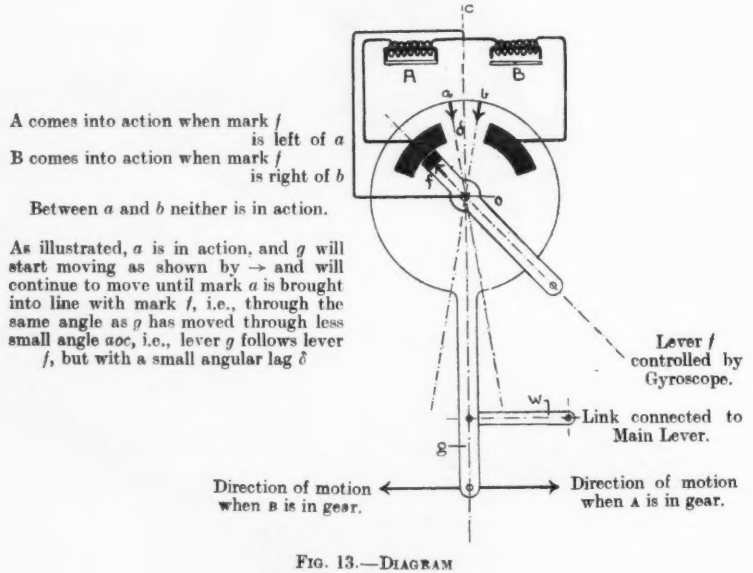

A comes into action when mark f is left of a
B comes into action when mark f is right of b
Between a and b neither is in action.
As illustrated, a is in action, and g will start moving as shown by → and will continue to move until mark a is brought into line with mark f, i.e., through the same angle as g has moved through less small angle aoc, i.e., lever g follows lever f, but with a small angular lag δ
C
A
B
a
b
d
f
o
Lever f controlled by Gyroscope.
W
Link connected to Main Lever.
g
Direction of motion when B is in gear.
Direction of motion when A is in gear.

FIG. 13.—DIAGRAM

The apparatus is designed so that the motion of the main lever v is proportional to the angle of precession. To understand this refer to the diagrammatic sketch, in which, for convenience, the contacts are not arranged as on the actual apparatus, but are shown on a flat disc. There will be seen to be other variations also. The diagram serves well to show the method which is somewhat of the same nature as the steam valve opening mechanism on some large engines, or the steering gear on ships, *i.e.*, the object is to make the motion of the main control lever fed with power (from the engine in our case), to follow exactly the motion of a small lever controlled by hand or otherwise (in our case gyroscopically controlled).

We have seen that the main lever v is moved to the left or right according as magnet n_1 or n_2 of the clutch is excited, or we will say according as A or B is in gear, A or B being the two sides of the clutch.

Now look at the diagram: lever f, carrying an electrical contact piece, is rigidly attached to the frame carrying the gyroscope.

Suppose the machine begins tilting to the right the frame at once precesses, and the lever *f* is carried over to the position shown, thereby making the contact which throws clutch A into gear. This causes the main lever to be drawn to the left. But the arm *g*, which is what is often called a floating lever, carrying the contacts is rigidly connected to the main lever *v* by the steel link W, so that the lever *g* follows the main lever. When G has thus turned through an angle *f c a* the contact is broken, and the main lever *v* and also G stop, *i.e.*, the main lever and G always follow up the gyroscope arm *f* but lag behind an amount—small angle due to the difference in length of the gap between contacts 1 and 2, and the width of the contact *f*. As the contacts 1 and 2 are each adjustable the angle can be made as small as we like, or it can be made of such a size as to introduce a definite amount of "delay" in the action, in which case the automatic control does not work if the rate of tilt is smaller than some settled amount.

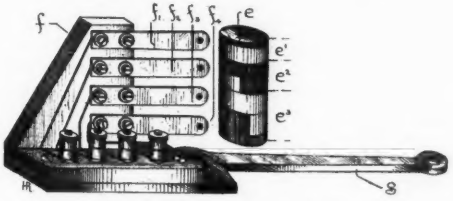


FIG. 14.—COMMUTATOR

An examination of the illustration of the commutator will show that the cylindrical part of this consists of three circular portions, the top one, which is fixed into the frame, containing a continuous metal ring fed from spring *f*, connected to + of battery, and to which is also attached one of the gyroscope motor leads. The second portion, which is in metallic contact with the third, contains the metal segment corresponding to 1 in the diagram, and against this presses f_2 connected to A. The third portion contains

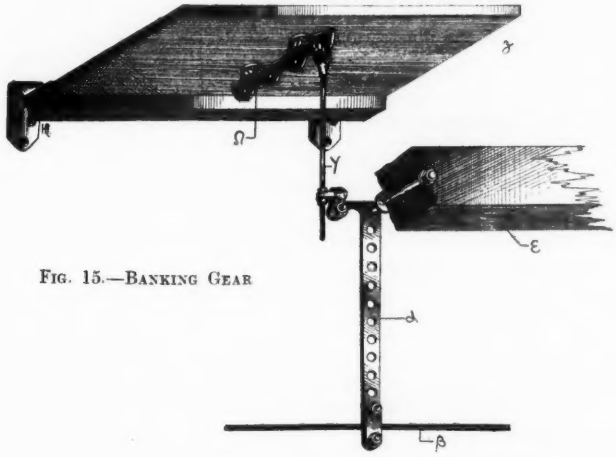


FIG. 15.—BANKING GEAR

a continuous ring fed from — of battery, and to which the other lead of the gyroscope is also permanently attached. There is also in this portion, and in metallic contact with the ring just mentioned, a metallic segment corresponding to E_2 of the diagram and connected through f_3 to B.

We thus see that we have an arrangement similar to that shown in the diagram with the addition that there are contacts for feeding the gyroscope.

The contacts were first made with carbon brushes against copper, but as the motion was hardly sufficient to keep them bright, and also the edges of the carbon were inclined to break off, they were replaced by platinum against platinum.

It will be noticed that the only outside work the gyroscope has to do is just to overcome the slight friction of its vertical pivot bearing, and the friction of the contacts; as these are very small, only a very small gyroscope need be used, the drum of the one shown is 4 in. diam., and this is more than sufficient. In order to make the amount of motion of the main lever vary with the rate of tilt, a spring is attached to the gyroscope arm *f*. Then since the precessional torque ($I\Omega\omega$) is directly proportional to ω, the rate of tilt of the machine, and also, owing to the known property of springs, the (precessional) angle is turned through, is proportional to (or a known function of) this torque, it follows that the lever *f*, and therefore also the main lever V, turns through an angle, which is proportional to the rate of tilt. Also it will be seen that the maximum range of these levers can be varied by adjustment of the stops *j*. The method of lubrication is seen in the illustrations.

There is one other point to go into, viz., the arrangements for turning. Without any further arrangements than those already described, when the machine turned it would try to keep on an even keel, and turning would become practically impossible, owing to the outward skidding, so some provision for "banking" has to be made. For this purpose the gyroscope, instead of being mounted rigidly on to the baseboard carrying the complete apparatus, is mounted on a small portion which is hinged to the main portion, the hinge lying fore and aft. Now the effect of the complete apparatus is to keep level the platform on which is mounted the gyroscope, and normally on straight flights this platform coincides with the rest of the baseboard and machine deck. But suppose when the rudder is put over a certain amount the curve taken is such as to require a banking angle α, then all we have to do is to arrange that when the helm is put over this amount the small gyroscope platform shall thereby be dipped through an angle $-\alpha$, relative to the main baseboard. This is effected by appropriately connecting the lever which operates the tilting of this small platform, to the cable which works the rudder, as can be seen in the illustration.

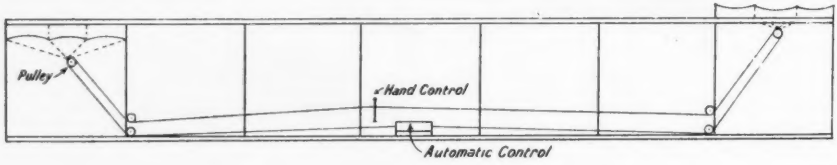


FIG. 16.—GENERAL ARRANGEMENTS ON AEROPLANE

For the independent application of hand control at any time the following simple contrivance is adopted. The cable working an aileron, instead of being attached directly to it, passes round a pulley which is attached to that aileron; one end of the cable then goes to the automatic control, and one to the hand control. As, also, each of these controls is irreversible, the working of one cannot affect the other.

In the apparatus shown the gyroscope and the clutch are each worked by the same battery, one of 10 volts, the cells being rather smaller than ordinary car size. Each uses about 1¼ ampères. The whole apparatus (except battery) is mounted on a baseboard with a light wooden cover with slot through which a main lever projects. This particular

set having been made for a machine in which the ailerons are subject to considerable unbalanced forces, is somewhat larger than would be necessary in most cases.

With regard to this species of control, it is interesting to note the opinion of M. Soreau, an eminent authority, which, as stated in the appendix to Professor Bryan's book, is that " he would leave his machine inherently unstable, and would use a gyroscope or other similar contrivance to counteract automatically this defect."

As to the behaviour of auto-mechanical devices in actual use, Captain Saunier has reported very favourably on the Doutre Stabilizer. A control, also very similar to the one I have described, but worked by a damped pendulum making contacts through mercury cups, has been tried in America and reported to be very successful. In both cases the pilots have laid stress on the fact that the apparatus recognises the necessity for action, and acts accordingly far sooner than any pilot can do so.

Discussion

In the discussion that followed Mr. Arch. R. Low gave his opinion that the gyroscopic method was the only solution of the problem of automatic stability, but how far Mr. Clarke and Mr. Johnstone had succeeded with the apparatus exhibited he was not prepared to say without critical examination.

Professor A. K. Huntington said that while extremely interested in what Mr. Clarke had shown them he was unable to add anything to the discussion, as for many years past and at the present time he had been working on the idea of automatic stability by fixed surfaces. Naturally he considered his method the best solution.

Mr. B. Melvill Jones raised an objection to the scheme, which he thought might be put into a dangerous position by the pilot using the hand control and righting the machine from a tilt more quickly than the gyroscope would do it, the result being that the gyroscopic righting action would continue after the machine had regained its equilibrium and a dangerous oscillation would be set up.

Mr. B. G. Cooper : I am very glad Mr. Clarke has made the apparatus which he has just described, as it, and the reasons which have led him to make it, go to drive another nail into the coffin of the " inherently stable " aeroplane. It is some time since I came to the conclusion that the limitations of this type of machine rendered it impossible for it to furnish a reasonably complete solution of the stability problem. We shall have to fly so often near the ground in gusty weather that the inherently stable machine will not have room in which to operate safely. All these machines have more or less frequently to move through a certain distance to recover balance when disturbed, and it is practically impossible always to ensure that obstacles shall not come within their " radius of recovery." I therefore pin my faith to auto-mechanical stability, and I am pleased to see how far Mr. Clarke has gone along this line of development.

Mr. Clarke's mechanism is divisible into two main parts, a relay mechanism and a switch for controlling its action. It would be a pity to consider these as being too much bound up the one with the other. Although Mr. Clarke's magnetic clutch is very nice, there is an obvious objection to using the engine power in the manner proposed for working the balancers. If the engine stops we are dished as far as auto-mechanical stability is concerned and must fall back on personal control at a time when we may want all our wits for attending to the engine. It seems to me that some form of compressed air reservoir or other similar system is preferable because it gives us a controlled store of energy, which moreover can be used when the engine stops.

Mr. Clarke has not mentioned one of the strongest arguments in favour of a mechanical relay system of working control organs, that is, the muscular exertion required to work large ones. Mr. Cody flies very big control planes, by " balancing " them, but one can easily conceive machines being required so large that if they were without mechanical control they would be somewhat like Crusoe's first canoe, unusable for lack of strength of the pilot.

There is one point which I don't think Mr. Clarke made enough of, and that is the quickness of action to which an automatic device can attain as compared with our quickest human actions. Our nerves can transmit messages at about 100 feet per second, that is to say, in a 5 feet 6 inch man a message from his toe would take about one-twentieth of a second to reach the brain. For this reason, a tall man would, other things being equal, be less quick at control than a short man. Only by a small fraction of a second it is true, but nevertheless some appreciable time, and when we are dealing with high speed machines any fraction of time is important. Similarly hand control might be more sensitive than foot control, and so on.

Consider what happens when we are balancing a machine personally. We become conscious of a tilt, either through our eyes, or that little "spirit level" which we have in our inner ear region, or some combination of our various sense organs. Well, a message goes to the brain that something has gone wrong, the brain thinks over the matter, and then a message goes out to some limb to do something. Now all this takes time, often vital time. Very often an inward and an outward message only is not sufficient, but several messages are necessary. We may therefore get a time lag of anything between one-twentieth to one-fifth or more of a second in setting controls into action. Now this question of time lag is really the vital point as regards controlled stability, either personal or mechanical. It is true that "reflex" action of our muscular system can occur without messages having to go through the brain, they go instead through some ganglionic centre from sense organ to muscle, and thus shorten the path, but even allowing for the acceleration of action due to this cause, we soon reach a limit to rapidity of manual control. If automatic control can reduce appreciably the time lag it will have accomplished a good deal. It must be remembered too, that human nerves are partially destroyed every time they are used, the damage done being of course repaired quickly. But in the course of a long flight, the nervous system becomes dulled, clogged with effects of hard work, and responds more and more slowly. On the other hand, a gyroscopic device should work equally quickly even after a prolonged effort.

There is a further avenue of thought to which these considerations lead, and that is this. We will suppose a device such as this to be perfected until there is practically no lag at all in it—then, if not before, we shall find the limitations of the organs which it actuates. Balancers, rudders, elevators and so on—all these are passive planes depending on the travel of the machine for their reactions. These reactions are consequently soon limited in rapidity and magnitude and I think there is no doubt that we shall have to come, for our stabilising system, to some controlled-propeller-thrust system. Mr. Short, in one of his latest patents, is going a step or two along this path in using the slip stream of the propellers to give an enhanced velocity over his control planes. Eventually we shall come, I feel certain, to controlled propeller thrust stability such as is general in insect flyers. The stability of the best of these, hawkmoths, hoverflies, &c., is of the neutral-equilibrium quick-control order, and is the most perfect stability I know. The more Mr. Clarke succeeds with this device the more will it be realised how essential it is to adopt a different propulsion and control system than the present one for flying machines.

THE CHAIRMAN raised the query as to what would happen when the machine received a motion about a vertical axis. He was very favourably impressed with the apparatus, but could not quite follow a motion in azimuth.

MR. T. W. K. CLARKE: Mr. Cooper raises the objection that in using the engine power for the control one is "dished" in the event of engine failure. In this particular apparatus the machine for which it was intended has a clutch and geared propellers. In the event of engine failure the propellers would still drive the apparatus—not so fast certainly.

With regard to muscular lag, this is a most important point, as Mr. Low has pointed out, owing to "reflex" actions. It may not be so much as Mr. Cooper would make out, but it exists, and we must note that there are two personal lags, *viz.*, lag in becoming

conscious of the necessity of action, and this is where those with actual experience give us to understand that the machine has a great advantage, and lag in the necessary muscular exertion after such consciousness.

Moreover, the actual motions themselves take time and with the increased power available for the apparatus this time can be reduced.

I was glad to hear Mr. Low say that he considered the "gyroscopic method" the only solution. With regard to some of my remarks about dihedral and anhedral angles, I did not wish them to be taken too literally; they were only particular instances.

Professor Perry raised the very important point of what happens when the machine receives a motion in azimuth, *i.e.*, about a vertical axis, owing, we will imagine, to a wind slam meeting the rudder. This was foreseen in the design. Practically nothing happens (unless the machine begins to tilt as well, then the usual operations take place), for as the gyroscope cannot precess about a horizontal axis it will give no gyroscopic resistance or precession owing to motion about a vertical axis, except that due to its ordinary inertia. This, coupled with the action of the small spring, giving it a tendency to keep central, causes the gyroscope to follow round with the machine except for a slight lag. This latter will bank the machine in the appropriate direction, but not for long.

The meeting then terminated.

THE AERODYNAMIC RESISTANCE OF BARS, STRUTS AND WIRES—II.

Made at East London College (University of London), for the Laboratory Committee of the Aëronautical Society of Great Britain

BY A. P. THURSTON, B.SC.

In the last communication on *The Aerodynamic Resistance of Bars, Struts and Wires —I., the various methods for finding the resistance of a bar by (a) "differences," (b) "side plates," and (c) "end guards" were investigated. The resistance of a circular bar was also determined. It was found that the "difference" method appeared to give the best results. This method of "differences" was therefore used by the author in determining the resistance of the bars given in this communication.

The method of differences applied to bars of various sections The method of differences having been found to apply to a circular rod of ·85 inch diameter it was necessary to investigate its suitability to bars of widely different sections.

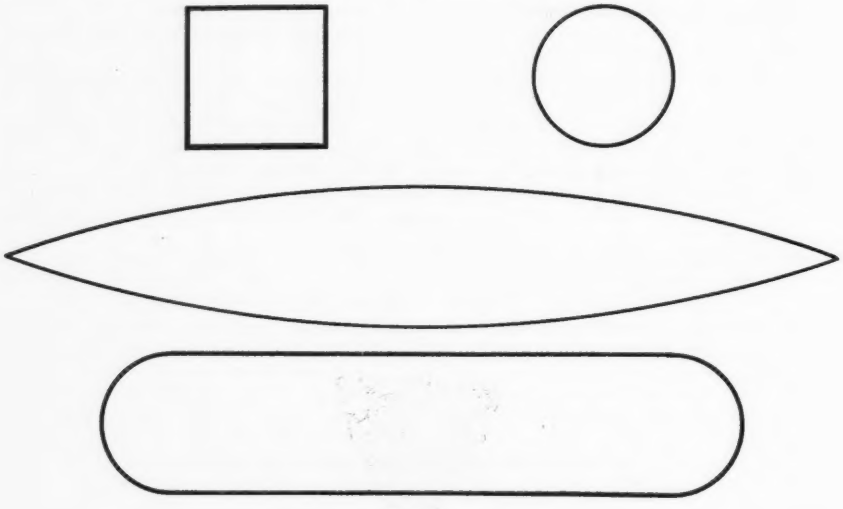


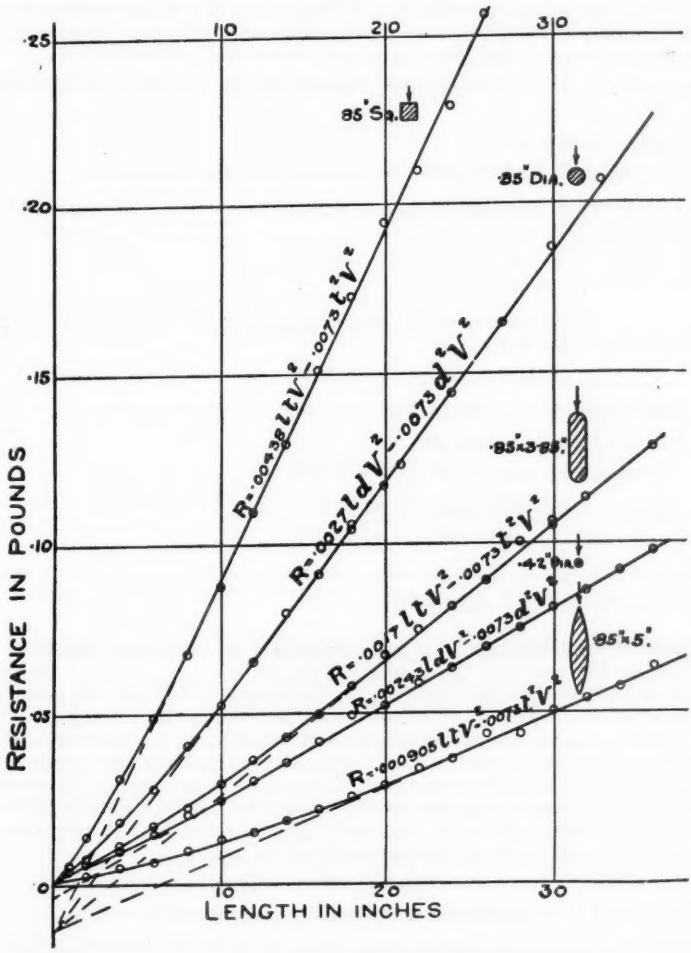
FIG. 1

For this purpose bars of the cross-sectional shape shown in Fig. 1 were constructed, each 36 inches long and ·85 inch thick, and the resistance determined. A length of 1 inch was cut off from each end and the resistance measured afresh at each step. The bars, when between 28 inches and 36 inches long, showed a considerable tendency to vibrate or oscillate in the tunnel. In order to prevent this, and thus obviate increase of resistance, if any, due to vibration, small abutment stops were placed in the tunnel to restrict the movement to within one-sixteenth of an inch. The results obtained are shown graphically in Fig. 2. From these curves it would appear that the method of differences may be applied equally well to bars of every cross section. They also incidentally demonstrate several most important points.

(1) First it will be noticed that the best straight lines to all the curves meet at a common point on the vertical axis below the origin. If this is so, it would appear to

* *The Aëronautical Journal*, April, 1911, No. 58, Vol. XV.

indicate that the total loss due to " end effect " or the slipping away of the air at the sides, is the same for bars of every section. Unfortunately this statement is not universally true, as some later experiments made with bars of other sections do not strictly agree with it.



(2) Secondly, it is seen that the straight lines, which have been drawn, satisfactorily represent the observations for all lengths of the square rod above 8 inches ; for all lengths of the circular rod about 10 inches ; for all lengths of the circular ended rectangle ·85 inch by 3·85 inches above 18 inches; and for all lengths of the pointed oval ·85 inch by 5 inches above 22 inches. This appears to indicate that the end effect is felt for a greater length of a bar with a long and narrow cross section than it is for a bar having a shorter cross section ; or in other words the blunter the bar the shorter the length over which " end effect " is felt.

(3) It follows, since an aeroplane is in effect a long thin body, that the loss, due to "end effect" on an aeroplane or aerocurve, is considerable. This loss could be largely obviated, as was proved in the previous communication, by a suitable arrangement of "side plates."

(4) It also follows, since the resistance of a strut or bar is less in proportion for a short bar than for a long bar, that it is advantageous in a flying machine to separate the struts up into as many sections or parts as possible with gaps between in order to increase the loss due to "end effect" and so to reduce the head resistance.

The equations of the straight lines representing the resistances of these bars were found to be :—

Circular rod 0·85 inch dia.

$$P = \cdot 0027\ ldV^2 - \cdot 0073\ d^2V^2.$$

Where V = velocity in miles per hour
d = diameter of rod in feet
l = length of rod in feet
P = resistance in pounds.

The air having a density of ·00122 at the standard conditions of 14·7 lbs. per sq. inch and temperature of 60° Fah.

Square rod 0·85 inch side.

$$P = \cdot 00438\ lt\ V^2 - \cdot 0073\ t^2V^2$$

Where V, l and P are as before, and

t = greatest thickness of the section in feet.

Circular ended rectangle 3·85 inches × ·85 inch

$$P = \cdot 0017\ lV^2 - \cdot 0073\ t^2V^2$$

Pointed oval 5 inches × ·85 inch.

$$P = \cdot 000905\ lt\ V^2 - \cdot 0073\ t^2V^2$$

Relation of the thickness of a bar to the constant in the equation of resistance Having determined the constant C in the equation of the resistance $R = mx + C$, the next point investigated was the connection between the thickness of a bar and this constant. For this purpose an approximately cylindrical enamelled rod, ·42 inch in dia. and 36 inches long was taken and its equation of resistance determined by the method of differences. These observations are plotted in Fig. 2, and the best straight line representing them has been drawn and produced to cut the vertical axis. The new value of the constant thus obtained is approximately a quarter of the constant for the bars ·85 inch thick, *i.e.*, the constant varies as the square of the thickness. The equations have therefore been put in this form. The equation for a thin enamelled rod ·42 inch dia. is

$$P = \cdot 00243\ ld\ V^2 - \cdot 0073\ d^2V^2$$

Where P, l, d, and V are as before and the density of air is ·00122.

This result which is somewhat less than the result previously determined, was obtained by suspending the bar from forked supports after the modifications in the tunnel, described later, were made.

Resistance of wire strainers or turnbuckles The resistance of turnbuckles of various sizes kindly supplied by Messrs. H. Rollet and Co., were determined. These turnbuckles were afterwards the subject of a series of tests for strength.* The results of these tests made under the supervision of Prof. D. A. Low, are given in the accompanying paper by Messrs. J. Brimelow and W. J. Mann. The dimensions of these specimens may also be obtained from the same paper.

* *The Aëronautical Journal*, Jan., 1912, No. 61, Vol. XVI.

Turnbuckle No. 1, Diameter of screw $\frac{1}{4}$ inch.

The resistance of turnbuckle No. 1 when screwed up to the maximum amount with the eyes to the wind and length over all equal to $5\frac{3}{4}$ inches is given by the equation

$$R = \cdot 0000319\ V^2$$

Where R = resistance in lbs.

V = velocity in miles per hour.

The resistance of turnbuckle No. 1 when screwed out to the maximum amount with $\frac{1}{4}$ inch of the screws in the nut, length over all $8\frac{9}{16}$ inches and eyes to wind is given by equation :

$$R = \cdot 0000437\ V^2.$$

Turnbuckle No. 2. Diameter of screw $\frac{5}{16}$ inch.

Resistance when screwed up to the maximum amount, length over all $4\frac{7}{8}$ inches and eyes to wind is given by equation :

$$R = \cdot 0000202\ V^2.$$

As before but screwed out to the maximum, length over all $7\frac{3}{8}$ inches.

$$R = \cdot 0000278\ V^2.$$

Turnbuckle No. 3. Diameter of screw $\frac{5}{32}$ inch.

Resistance when screwed up to the maximum amount, length over all $4\frac{1}{16}$ inches and eyes to wind :

$$R = \cdot 0000158\ V^2.$$

As before but screwed out to the maximum, length over all $6\frac{1}{4}$ inches :

$$R = \cdot 000021\ V^2.$$

The density of the air in all cases above was equal to ·00122. It was not considered necessary to determine the resistance of the smaller sizes Nos. 4 and 5.

Alterations to pitot tube, wind tunnel, balance and water gauge At this stage of the experiments Dr. Stanton of the National Physical Laboratory most kindly sent particulars of the pitot tube which he had found to give more satisfaction than the one before described in the author's paper in the AËRONAUTICAL JOURNAL, April, 1911.

The new device is shown in Fig. 3. The dynamic tube is the same as before, but the static tube is replaced by a hole flush with the wall of the tunnel.

The velocity from this device is given by the equation

$$V = \sqrt{2\,g\,h}$$

Where h is the difference in pressure, in the two tubes, in feet of air at the statical temperature and pressure in the tunnel

g is the acceleration due to gravity

V is the velocity in feet per sec.

At the same time the glass water gauge was replaced by one having vertical tubes of smaller diameter in order to make the gauge quicker in response.

In this arrangement

$$\frac{\text{lift of adjusting screw}}{\text{lift of vertical tubes}} = \frac{343}{325 \cdot 5}$$

$$\therefore \text{Pressure (in mm. of water)} = \text{lift of nut} \times \cdot 949$$

$$\therefore \text{H (ft. air)} = \frac{\text{Pres. (mm. water)}}{304 \cdot 8 \times \cdot 00122}$$

$$= \text{lift of nut} \times 2 \cdot 55$$

It should be stated that this water gauge was used for all the remaining experiments herein described, but it was found to give no better results than the one previously used. Experiments of this nature would be greatly simplified if a simpler and more accurate means than the Pitot tube for obtaining the velocity of the wind in the tunnel could be devised. Such a device is now being considered.

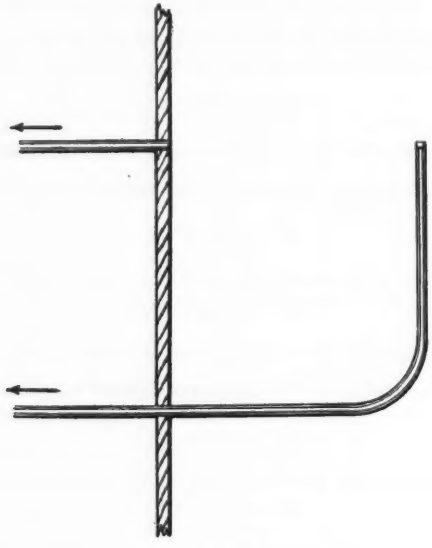


FIG. 3

While the above alterations were being made the balance was fitted with a fork for allowing the specimens to be suspended from the balance at two points.

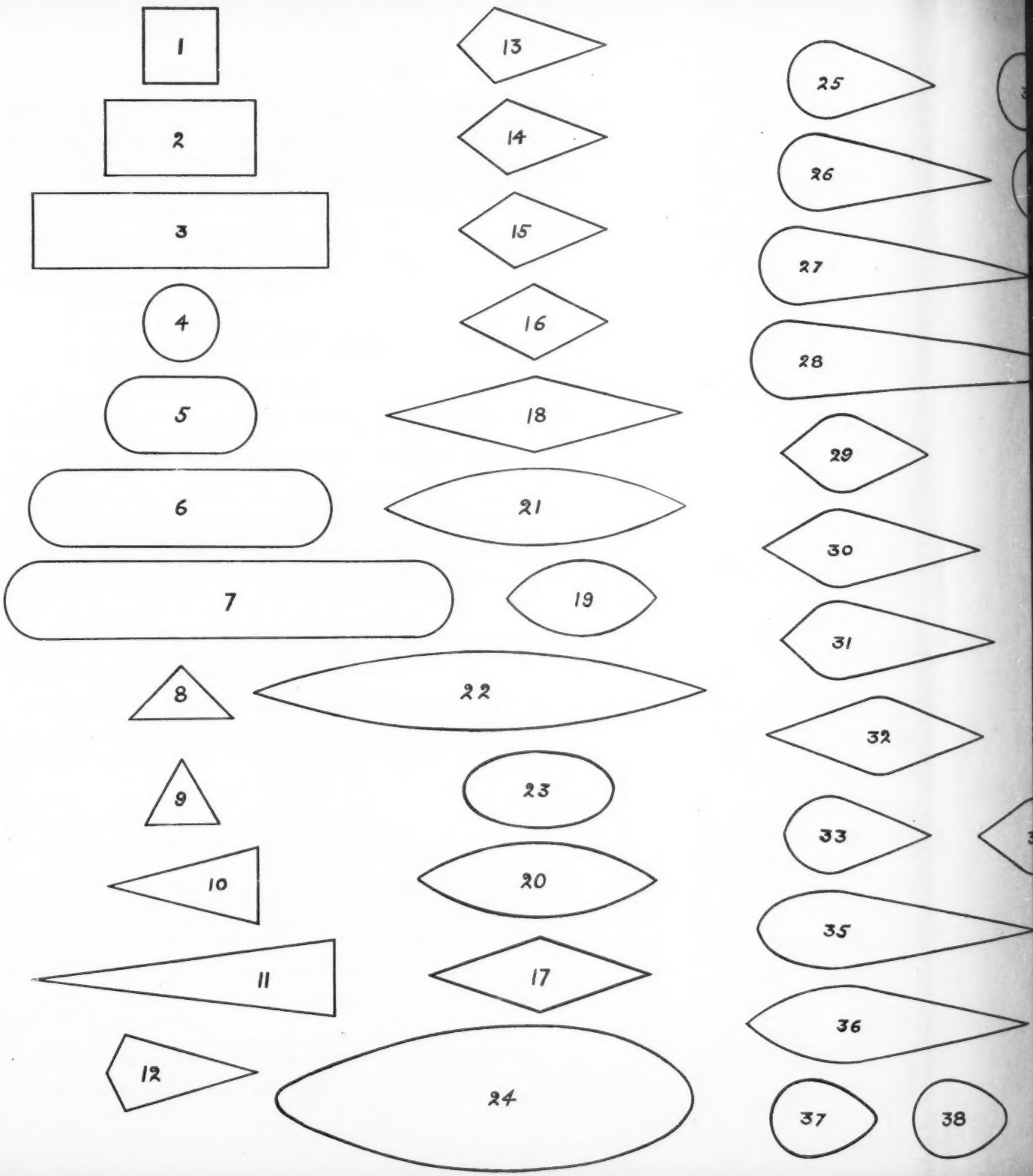
The balance was recalibrated and it was found that 1 cm. displacement of jockey weight = ·002637 lb.

The gap between the short and long portions of the tunnel was also filled in, small slots only being left to allow the forked ends of the balance to project into the tunnel.

This new method of mounting the specimen was made in order to eliminate any errors due to side thrust caused by want of symmetry in the bars to be tested, and in order that the bars might automatically adjust themselves to the position of minimum resistance.

Method of obtaining the equations of resistance A large number of bars having sections of various geometrical constructions and all $\frac{5}{8}$ inch thick as shown in Figs. 4A and 4B had been obtained. They were all made $\frac{5}{8}$ inch thick because this thickness is well within the limits of the tunnel.

Templets of the various shapes had been prepared and supplied to the carpenters. The bars which they made showed grave departure from the dimensions supplied and in some cases want of symmetry. Three different carpenters and pattern makers of the highest skill were tried with no better results. It was therefore evident that bars for the purposes of experiments such as these should be machine cut, but want of funds and time prevented this being done. The author would suggest that this should be done in future experiments since it has been proved that apparently trivial differences of shape may, in some cases, greatly alter the resistance obtained. Each particular section was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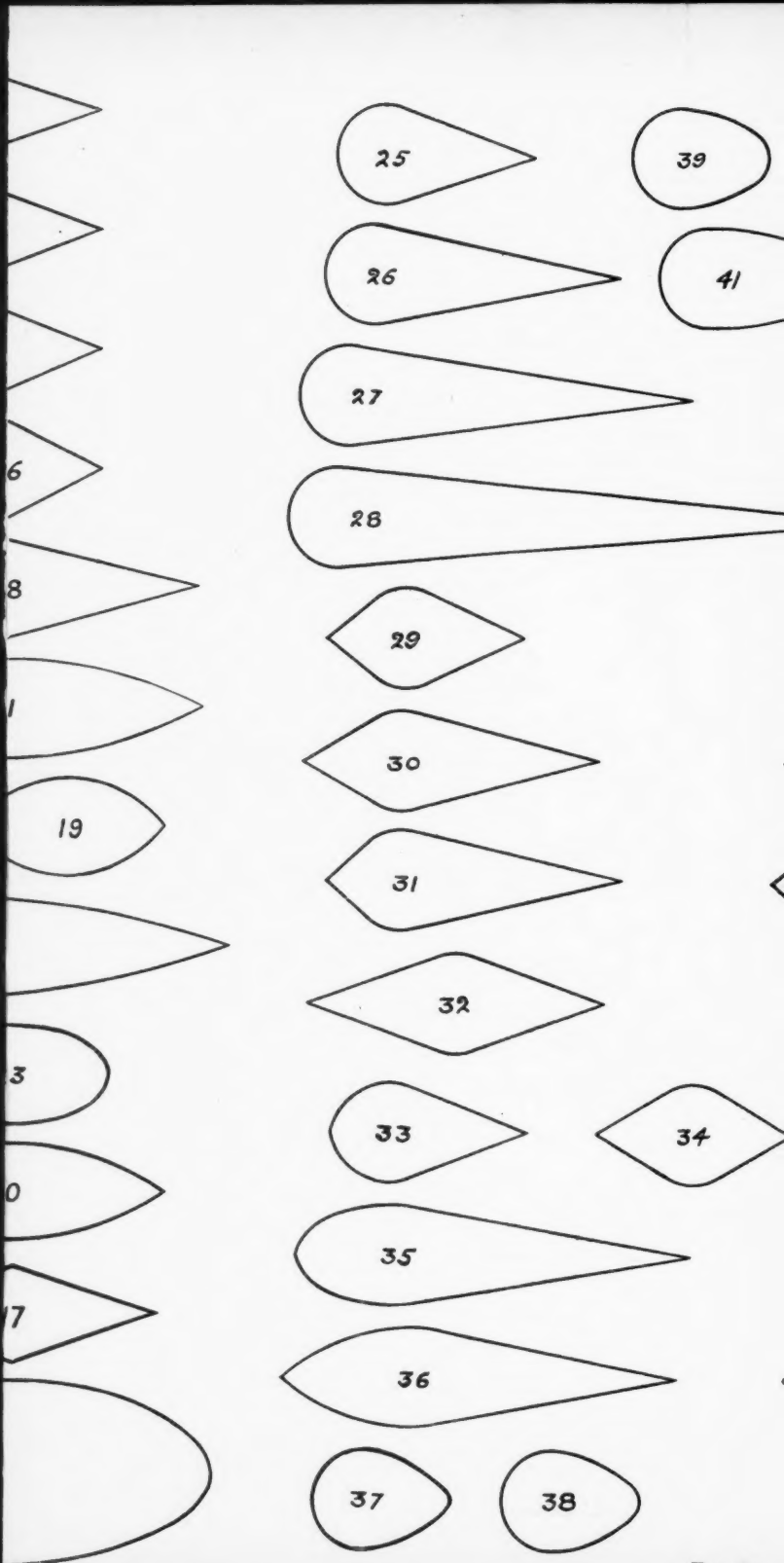


FIG. 4B

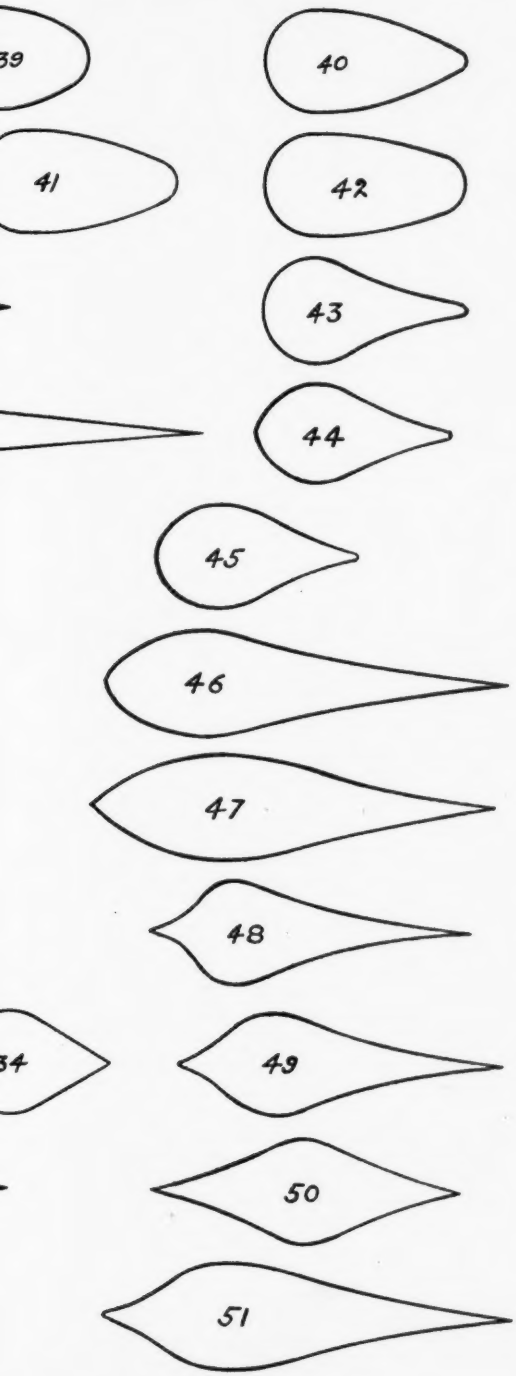


FIG. 4B

represented by bars of 12 inches, 18 inches and 30 inches length, and in some cases by additional lengths of 6 inches and 24 inches. The resistances of all these specimens reduced to the standard velocity of 20 miles per hour were determined, in some cases by many experiments, and the results were plotted. A point below the origin corresponding with the thickness of $\frac{5}{8}$ inch, and as previously justified, was taken and, for the sake of uniformity, the best straight lines to the various curves were drawn from this point and their equations determined.

These equations were therefore all of the order

$$R = K l t V^2 - \cdot 0073\, t^2 V^2$$

Where K is the constant of the section

R = resistance in pounds

V = velocity in miles per hour

t = maximum thickness of bar at right angles to the current of air in feet

l = length of bar in feet.

The air in all cases had a density of ·00122.

If the bar is infinitely long or in fact reasonably long the value of the expression $\cdot 0073\, t^2 V^2$ may be neglected and the equation becomes $R = K\, l t\, V^2$.

The resistance of bars In Tables 1, 2 and 3 the constants K for various bars are given.

To enable the relative resistances of the various bars to be seen at a glance the resistance modulus Z was calculated in terms of the resistance of a square bar. Thus the modulus Z of the square section is given as unity. The Z's for all other sections are in terms of this, the bar having the least resistance being the one with the smallest value of Z.

The column W gives the weight of the bars in terms of the weight of an equal length of a square bar of the same thickness. Thus the lightest bar is the one having the smallest value of W.

The next column KW gives the product of weight and resistance in terms of the product KW for the square bar. The bar having the smallest value is therefore the best one as regards the combination of weight and resistance.

The column $\frac{K}{S}$ was calculated in order to set forth the value of a bar in terms of the resistance and strut value S. The strut value S of a spruce bar as given by Prof. D. A. Low in his paper published in the AËRONAUTICAL JOURNAL, April, 1911, is

$$S = 12{,}832 \frac{I}{l^2}$$

Where S is the crippling load or strut value.

I is the least moment of inertia of the section.

l the length of the strut.

The value of S for all the bars are tabulated in terms of the value for the square. In order, therefore, to find the crippling load of any strut having a shape shown in Figs. 4A, 4B it is only necessary to calculate the crippling load of a square strut from Prof. Low's formula and to multiply this result by the strut value of the section. The values of $\frac{K}{S}$ in terms of the value of $\frac{K}{S}$ for the square bar are also tabulated. The bar having the smallest value in this column is therefore the best one as regards the combination of resistance and its value as a strut.

In a similar way the column $\frac{KW}{S}$ sets forth the value of a bar as regards the combination of weight, resistance and strut value.

By extending this process not only for bars and struts but also for other parts of a flying machine a most convenient table of reference would be obtained which should be of the utmost value to designers and which at the same time should greatly reduce the labour involved in design.

Table I. gives the values of the constant K &c. for bars having the "blunt end section" to the wind. Blunt end sections are to be taken as meaning all sections in which the maximum thickness occurs between the end facing the wind and the middle of the section. Thin end sections are the reverse of the above.

Table II. gives the values of the constant K &c. for the same bars as in Table I., but with the thin end to the wind.

TABLE No. I.

Blunt end to wind.

Description and number of specimen.	K	Z	W	S	ZW	$\frac{Z}{S}$	$\frac{ZW}{S}$
Width of all specimens = $\frac{5}{8}$-in.							
1 Square (side to wind) ..	·00441	1	1	1	1	1	1
8 Right angled triangle ..	·0045	1·02	·25	·042	·256	24·5	6·13
9 Equilateral triangle ..	·00448	1·02	·432	1·94	·441	5·26	2·27
10 Isosceles triangle ..	·00428	·973	1·00	·50	·973	1·946	1·946
11 ,, ,, ..	·00342	·776	2·00	1·0	1·552	·776	1·552
12 Quadrilateral	·00361	·818	1·00	·50	·818	1·636	1·636
13 ,, ...	·00318	·721	1·00	·50	·721	1·442	1·442
14 ,, ...	·00278	·630	1·00	·50	·630	1·26	1·260
15 ,, ...	·00254	·576	1·00	·50	·576	1·15	1·150
24 Oval*	·0009	·203					
25 Circular based triangle ..	·00192	·435	1·07	·708	·465	·615	·658
26 ,, ,, ,,	·00204	·462	1·66	·960	·767	·481	·797
27 ,, ,, ,,	·00208	·472	2·16	1·18	1·018	·4	·863
28 ,, ,, ,,	·00127	·288	3·17	1·725	·915	·167	·53
29 Oval, pointed ends ..	·00215	·488	1·075	·656	·524	·744	·799
30 ,, ,, ,, ..	·00144	·325	1·62	·897	·526	·362	·586
31 ,, ,, ,, ..	·00157	·356	1·643	·913	·585	·390	·641
33 ,, ,, ,, ..	·00188	·426	1·095	·734	·467	·581	·636
35 ,, ,, ,, ..	·00087	·198	2·35	1·335	·466	·148	·349
36 ,, ,, ,, ..	·00096	·218	2·24	1·298	·488	·168	·376
37 Oval	·00246	·556	1·018	·687	·566	·810	·825
38 ,, ...	·00232	·526	1·038	·703	·546	·749	·777
39 ,, ...	·00239	·542	1·043	·742	·565	·730	·762
40 ,, ...	·00166	·376	1·435	·945	·540	·398	·571
41 ,, ...	·00183	·415	1·52	1·008	·631	·412	·631
42 ,, ...	·00159	·360	1·57	1·087	·565	·331	·52
43 Fish Shape	·00195	·442	1·075	·685	·475	·645	·694
44 ,, ,, ...	·00234	·530	1·050	·680	·556	·780	·818
45 ,, ,, ...	·00208	·472	1·084	·826	·511	·571	·618
46 ,, ,, ...	·00138	·313	2·21	1·164	·692	·269	·594
47 ,, ,, ...	·00127	·288	2·43	1·320	·700	·218	·530
48 ,, ,, ...	·00220	·498	1·415	·711	·705	·703	·992
49 ,, ,, ...	·00188	·425	1·55	·700	·660	·607	·943
51 ,, ,, ...	·00157	·356	2·23	1·14	·795	·313	·697

* This bar was $1\frac{1}{8}$ inches thick.

Table III. gives the values of the constant K &c. for bars which are symmetrical about the maximum thickness; the values of K for both methods of presentation being therefore identical.

It will be seen by reference to the values of K in Tables I. and II. that in most cases the resistance of a bar is less with the thin end to the wind than with the blunt or thick end. This appears to be contrary to general belief, which is to the effect that all bars have least resistance with the blunt end to the wind. The author, hesitating to publish matter which appears to be so contrary to existing knowledge, has delayed the publication of this fact for twelve months and has, in the meantime, further investigated the point. All the weight of evidence and proof being in support of these results it was felt that publication should not be delayed further.

TABLE No. II.

Thin end to wind.

Description and number of specimen.	K	Z	W	S	ZW	$\frac{Z}{S}$	$\frac{ZW}{S}$
8 Right angled triangle ..	·00333	·755	·25	·0417	·189	18·1	4·53
9 Equilateral triangle ..	·00277	·628	·432	·194	·272	3·23	1·395
10 Isosceles triangle ..	·00209	·474	1·00	·50	·474	·948	·948
11 ,, ,, ..	·00167	·379	2·00	1·0	·757	·379	·757
12 Quadrilateral	·00226	·512	1·00	·50	·512	1·024	1·024
13 ,, ...	·00247	·560	1·00	·50	·560	1·12	1·12
14 ,, ...	·00242	·549	1·00	·50	·549	1·097	1·097
15 ,, ...	·00238	·54	1·00	·50	·54	1·079	1·079
24 Oval*	·00116						
25 Circular based triangle	·00204	·463	1·07	·708	·495	·654	·7
26 ,, ,, ,,	·00153	·347	1·66	·960	·575	·362	·601
27 ,, ,, ,,	·00160	·363	2·16	1·18	·783	·307	·664
28 ,, ,, ,,	·00131	·296	3·17	1·725	·938	·172	·544
29 Oval, pointed ends ..	·00198	·448	1·075	·656	·481	·682	·733
30 ,, ,, ,,	·0015	·339	1·62	·897	·549	·378	·613
31 ,, ,, ,,	·00160	·363	1·643	·913	·595	·397	·653
33 ,, ,, ,,	·00194	·440	1·095	·734	·482	·6	·657
35 ,, ,, ,,	·00132	·3	2·35	1·335	·704	·224	·527
36 ,, ,, ,,	·00128	·290	2·24	1·298	·651	·224	·502
37 Oval	·00195	·441	1·018	·687	·449	·642	·654
38 ,, ...	·00194	·440	1·038	·703	·457	·626	·650
39 ,, ...	·00187	·423	1·043	·742	·442	·571	·596
40 ,, ...	·00147	·333	1·435	·945	·478	·353	·505
41 ,, ...	·00146	·331	1·52	1·008	·503	·328	·499
42 ,, ...	·00142	·322	1·57	1·087	·506	·296	·466
43 Fish shape	·00226	·512	1·075	·685	·549	·748	·804
44 ,, ,, ...	·00219	·497	1·050	·680	·521	·730	·767
45 ,, ,, ...	·00205	·464	1·084	·862	·503	·538	·583
46 ,, ,, ...	·00140	·317	2·21	1·164	·701	·272	·603
47 ,, ,, ...	·00148	·335	2·43	1·320	·813	·254	·616
48 ,, ,, ...	·00199	·450	1·415	·711	·638	·633	·896
49 ,, ,, ...	·00188	·425	1·55	·700	·66	·607	·943
51 ,, ,, ...	·00156	·353	2·23	1·14	·787	·31	·690

* This bar was $1\frac{1}{8}$ inches thick.

TABLE No. III.

Symmetrical bars.

Description and number of specimen.	K	Z	W	S	ZW	$\frac{Z}{S}$	$\frac{ZW}{S}$
1 Square (side to wind) ..	·00441	1	1	1	1	1	1
2 Rectangle	·00320	·726	2·0	1·975	1·452	·368	·735
3 „	·00254	·576	4·0	3·92	2·304	·147	·588
4 Circle*	·0027	·613	·785	·590	·481	1·038	·815
5 Parallel sides semi ⊙ ends	·00157	·356	1·785	1·59	·636	·224	·4
6 „ „ „	·00185	·42	3·785	3·59	1·59	·117	·443
7 „ „ „	·00184	·418	5·785	5·59	2·415	·075	·432
16 Rhombus..	·00238	·540	1·0	·5	·540	1·08	1·08
17 „	·00172	·389	1·5	·738	·584	·526	·792
18 „	·00132	·298	2·00	1·0	·596	·298	·596
19 Double Segment ..	·00189	·429	1·385	·921	·593	·465	·644
20 „ „ ..	·00133	·302	2·13	1·385	·643	·218	·463
21 „ „ ..	·00113	·256	2·68	1·78	·684	·144	·384
22 „ „ ..	·00112	·254	4·10	2·865	1·04	·089	·364
23 Ellipse	·00177	·400	1·565	1·18	·626	·339	·53
32 Oval, pointed ends ..	·00166	·376	1·560	·850	·587	·443	·69
34 „ „ „	·00215	·487	1·07	·662	·521	·736	·787
50 Fish shape	·00185	·419	1·60	·764	·671	·549	·873
1 Square (diagonal to wind)	·00347	·787	·50	·25	·393	3·14	1·57

* From rod ·85 inch diameter.

It may therefore be stated as a general fact that *the resistance of most bars is least with the thin end to the wind with the exception of a few shapes which are the forms for minimum resistance.*

In these exceptional cases the resistance is least with the blunt end to the wind.

By a careful consideration of the values of K for the various bars the forms for which the change occurs are seen, and it appears probable that the hump in Mr. Dines' curve for the pressure on an inclined plane is largely responsible for the result. Want of space prevents a fuller discussion of these results and the effect of shape upon resistance.

Table IV. gives the values of K, W, S for the bars ·85 in. thick, shown in Fig. 1.

TABLE No. IV.

Specimen.	K	Z	W	S	ZW	$\frac{Z}{S}$	$\frac{ZW}{S}$
Square ·85 in. (side to wind.)	·00438	1	1	1	1	1	1
Circle ·85 in.	·0027	·616	·785	·590	·484	1·044	·82
⬭ 5 in. × ·85 in. oval, pointed ends	·00091	·2065	4·015	2·82	·830	·0732	·295
⬭ 3·85 in. × ·85 in. Parallel sides, semi-circular ends	·0017	3·875	3·785	3·59	1·468	·108	·408

Comparison with the work of previous experimenters The following formulæ for bars of various sections were deduced from experimental data obtained by Sir Hiram Maxim over twenty years ago, and published in the author's book, *Elementary Aeronautics*, page 29.

Square bar, 2 inch side, with face normal to wind

$$R = \cdot 0039\ AV^2.$$

Where A = area in square feet of one face and corresponds with the product *lt*.

V = velocity in miles hour.

Square bar, 2 inch side, with one diagonal in line with the wind

$$R = \cdot 0041\ AV^2.$$

When this is reduced in terms of the area of a section through a diagonal we get

$$R = \cdot 0029\ A^1V^2.$$

Round bar 2 inch diameter.

$$R = \cdot 0022\ AV^2.$$

Pointed bar 12 inches by 2 inches, shown in Fig. 5.

$$R = \cdot 00016\ AV^2.$$

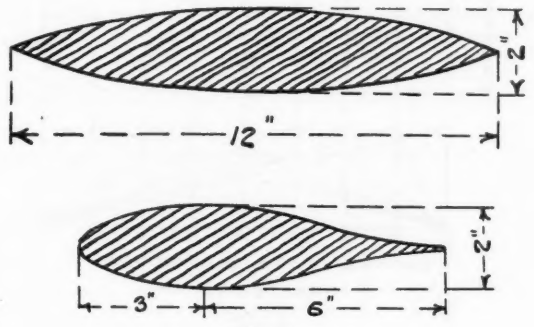


FIG. 5

Fish-shaped body 9 inches by 2 inches shown in Fig. 5, the maximum thickness being $\frac{1}{3}$ depth from the front edge.

Thick end to wind.

$$R = \cdot 000195\ AV^2.$$

Thin end to wind.

$$R = \cdot 0005\ AV^2.$$

The density of the air is not given for the above results.

The corresponding values of K herein determined for bars $\frac{5}{8}$ inch thick are :—

		Maxim.
Square (normal)	K = ·00441	·0039
Square (diagonal)	K = ·00347	·0029
Circle	K = ·0027	·0022
Pointed bar (6 to 1)	K = ·00112	·00016

Maxim's result is apparently about $\frac{1}{7}$ of the actual value in the case of the pointed bar.

Relation of resistance to the shape, position of the maximum thickness, and the depth of the cross-section of a bar The bars had been constructed, as previously explained, along certain geometrical lines, in order that some insight into the relation of resistance, shape, position of maximum thickness, and depth, might be obtained from the experimental results.

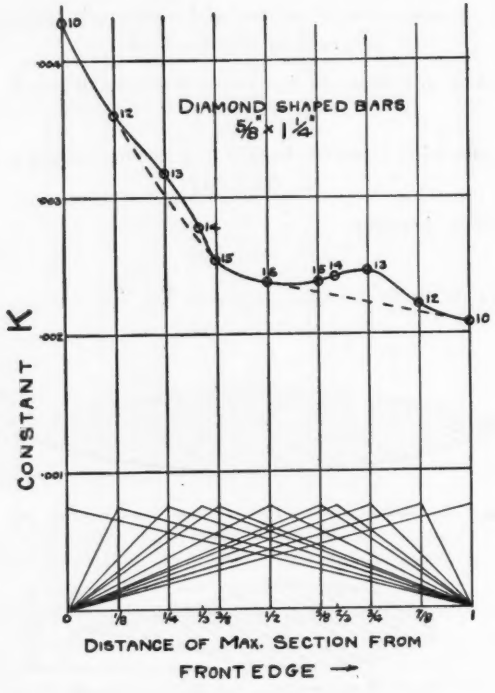


FIG. 6

Fig. 6 shows graphically the values of K for diamond-shaped bars $1\frac{1}{4}$ inches deep and $\frac{5}{8}$ inch thick formed with straight lines and sharp angles, the distance of the maximum thickness from the front edge being varied from the limiting case of O, for a triangle with the base to the wind, through the values, $\frac{1}{8}$, $\frac{1}{4}$, $\frac{1}{3}$, $\frac{3}{8}$, $\frac{1}{2}$, $\frac{5}{8}$, $\frac{2}{3}$, $\frac{3}{4}$ and $\frac{7}{8}$ the depth to the limiting case of 1 for a triangle with the apex to the wind. The curve divides into two portions, each having a hump. The shape having the least resistance of the whole series is the triangle with the apex to the wind, but another slightly higher minimum value is given by the curve for distances of the maximum thickness from the front edge of from $\frac{1}{2}$ to $\frac{5}{8}$ the depth. The resistance rapidly increases as the maximum thickness approaches the front edge.

The upper curve in Fig. 7 shows graphically the values of K for a similar diamond-shaped body $1\frac{1}{4}$ inches deep formed by drawing tangents to a circle $\frac{5}{8}$ inch diameter, the centre of the circle varying from the limiting position of $\frac{1}{4}$ through the values of $\frac{1}{3}$, $\frac{1}{2}$ and $\frac{2}{3}$ to the limiting case of $\frac{3}{4}$ the depth from the front edge. The ratio $\frac{\text{depth}}{\text{thickness}}$ therefore $=2$. The minimum resistance is given by the section having the maximum thickness $\frac{2}{3}$ the depth from the front edge. The circular based triangle used in

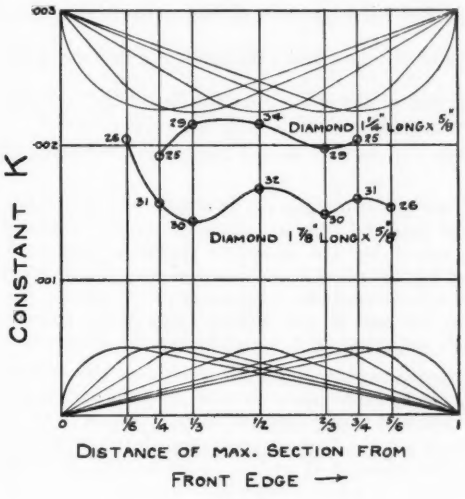


FIG. 7

this curve was not well formed, it being rather more pointed than a true circle, and hence too much reliance should not be placed upon the limiting values.

The same remark applies to the limiting values of the lower curve in Fig. 7, the circular base of the bar being somewhat flatter than it should have been.

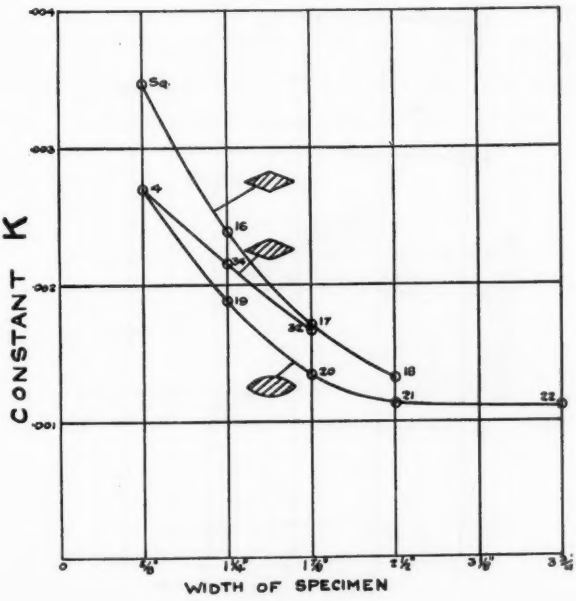


FIG.

The lower curve in Fig. 7 is for a series of bars similarly shaped to those in the upper curve, but with a depth of $1\frac{7}{8}$ inches and a thickness of $\frac{5}{8}$ inch, *i.e.*, a ratio of $\frac{\text{depth}}{\text{thickness}} = 3$. The distance of the centre of the circle from the front edge was varied from the limits $\frac{1}{6}$ to $\frac{5}{6}$ the depth through the values $\frac{1}{4}$, $\frac{1}{3}$, $\frac{1}{2}$, $\frac{2}{3}$ and $\frac{3}{4}$.

The least resistance is given by a section having the maximum thickness $\frac{1}{3}$ the depth from the front edge.

The values of K are shown graphically in Fig. 8 for diamond-shaped bars, circular centre diamond-shaped bars and pointed bars made up of two arcs of circles, the depths of the sections being varied, but the maximum thickness being kept a constant $\frac{5}{8}$ inch. The upper curve gives the values of K for diamond-shaped bars formed of straight lines with sharp angles and symmetrical about the maximum thickness, the depth being varied from $1{\cdot}414 \times \frac{5}{8}$ inch in the case of the square through $1\frac{1}{4}$ inches and $1\frac{7}{8}$ inches to $2\frac{1}{2}$ inches, *i.e.*, through the values of 2, 3 and 4 times the thickness.

The lower and approximately parallel curve shows the values of K for pointed sections each $\frac{5}{8}$ inch thick, made up of arcs of circles struck from points on the line passing through the centre of the section and at right angles to the wind. The limiting case is the circle which has the highest resistance of the series. The depth is varied from $\frac{5}{8}$ inch through the values of $1\frac{1}{4}$ inches, $1\frac{7}{8}$ inches, and $2\frac{1}{2}$ inches to $3\frac{3}{4}$ inches, *i.e.*, through the values of 1, 2, 3, 4 and 6 times the thickness.

The intermediate curve shows the values of K for the circular centre diamond section. The limiting case is the circle. The depth is varied from $1\frac{1}{4}$ inches to $1\frac{7}{8}$ inches, *i.e.* through the values of 1, 2 and 3 times the thickness. This curve would approximately coincide with the curve for the true diamond for all values above 4 times the thickness.

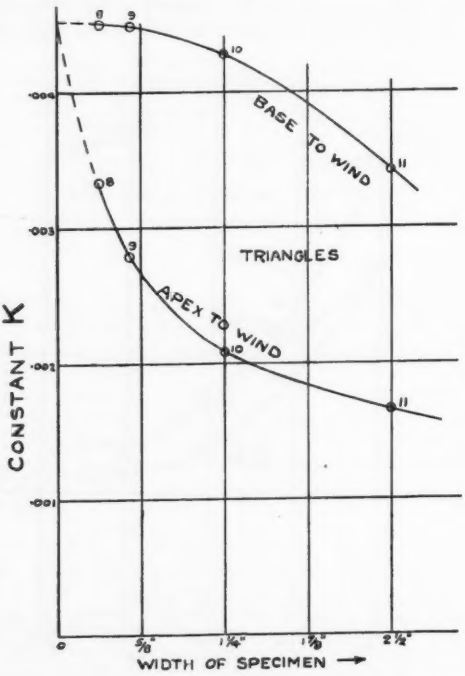


FIG. 9

The resistance in all cases rapidly decreases as the depth is increased, but this decrease is much more rapid with bars of small depth.

Fig. 9 shows graphically the values of K for triangular bars with the apex and the base to the wind, the base being always $\frac{5}{8}$ inch, and the height or depth being varied from $\frac{5}{16}$ inch through the values of $1\frac{1}{4}$ inches, $1\frac{7}{8}$ inches and $2\frac{1}{2}$ inches, *i.e.*, through the values of $\frac{1}{2}$, 2, 3 and 4 times the base. The resistance of all the triangles is much greater with the base to the wind than with the thin end to the wind.

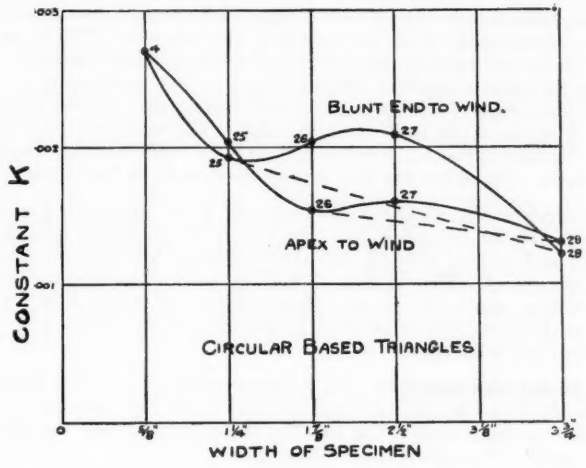


FIG. 10

Fig. 10 shows the curves of K for circular based triangles all of $\frac{5}{8}$ inch maximum thickness. The limiting value is the circle. The depth is varied from $\frac{5}{8}$ inch through the values of $1\frac{1}{4}$ inches, $1\frac{7}{8}$ inches, $2\frac{1}{2}$ inches to $3\frac{3}{4}$ inches, *i.e.*, through the values of 1, 2, 3, 4 and 6 times the thickness. The curve having the greater portion of its length above

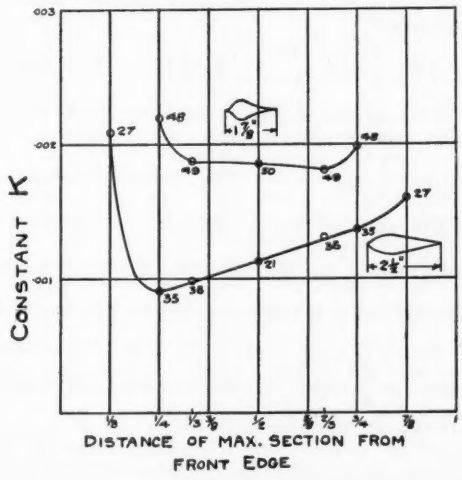


FIG. 11

the other gives the values of K for the bars with the blunt ends to the wind and the lower curve, the values of K when the thin ends are to the wind. Both the curves appear to have humps on the lower portions, but whether this is due to the hump of Mr. Dine's curve, or to inaccuracies due to the faulty construction of the bars previously remarked upon, is not clear.

The values of K for fish-shaped sections are shown in Fig. 11.

The values of K in the upper curve are for bars each $\frac{5}{8}$ inch thick and $1\frac{7}{8}$ inches deep, the section being formed by a circle $\frac{5}{8}$ inch diameter at a certain distance from the front edge touched by concave arcs which are tangent to the long axis. The carpenters found considerable difficulty in making these bars and some of them showed considerable departure from the section desired. The limiting case is a bar having a circular end. The position of the centre of the circle was varied from $\frac{1}{4}$ through the values $\frac{1}{3}$, $\frac{1}{2}$ and $\frac{2}{3}$ to the value $\frac{3}{4}$ of the depth from the front edge. The resistance appears to remain approximately constant between the values of $\frac{1}{3}$ to $\frac{2}{3}$, *i.e.*, these bars have approximately the same resistance with either the thin or the blunt ends to the winds.

The lower curve in Fig. 11 shows the values of K for bars each $\frac{5}{8}$ inch thick and $2\frac{1}{2}$ inches deep, the section being formed by arcs of circles struck from a line perpendicular to the long axis and passing through one apex, tangents being drawn from the other apex to touch these arcs. The position of the line from which the arcs are struck is varied. The limiting case is the circular based triangle $2\frac{1}{2}$ inches deep. The position of the maximum thickness was varied from $\frac{1}{8}$ inch through the values $\frac{1}{4}$, $\frac{1}{3}$, $\frac{1}{2}$, $\frac{2}{3}$, $\frac{3}{4}$ to $\frac{7}{8}$ of the depth from the front edge.

The shape having least resistance The resistance of the type of bar shown in Fig. 11 is less with the blunt end to the wind and is least when the maximum thickness is $\frac{1}{4}$ the depth from the front edge. The bar No. 35 appears to have the least resistance of all the bars tested. Its equation of resistance is

$$R = \cdot 000874\ lt\ V^2 - \cdot 0073\ t^2V^2.$$

It is obvious that there are shapes having a still smaller resistance than this bar, but the investigation of these shapes must be left for a future occasion.

The bar No. 24 kindly supplied by Messrs. T. W. K. Clarke and Co. appears to have an almost equally small resistance, but is somewhat outside the limits of the tunnel.

This bar is formed of two grooved parts glued together along the main axis and spaced apart by a centre web.

The columns for ZW, $\frac{Z}{S}$ and $\frac{ZW}{S}$ in Tables I., II., and III., give some interesting results. Thus in Table No. I. for bars with the blunt end to the wind the right angled triangle No. 8 gives the minimum value for ZW and the maximum values for $\frac{Z}{S}$ and $\frac{ZW}{S}$. The isosceles triangle No. 11 gives the maximum value for ZW and the double pointed pear-shaped body No. 35 gives the minimum value for $\frac{Z}{S}$ and $\frac{ZW}{S}$. The value of ZW for this bar is also satisfactorily small. This bar is therefore one of the most satisfactory all-round bars tested.

In Table No. II. the right angle triangle No. 8 has the minimum value for ZW and the maximum value for $\frac{Z}{S}$ and $\frac{ZW}{S}$. The circular based triangle No. 28 has the maximum value for ZW and the minimum value for $\frac{Z}{S}$.

The oval No. 42 has the minimum value for $\frac{ZW}{S}$.

In Table No. III. for symmetrical bars, the maximum value of ZW is given by the circular ended rectangle No. 7 and the minimum value by a square with the diagonal to the wind. The maximum value of $\frac{Z}{S}$ is also given by the same square, and the minimum value by the circular ended rectangle No. 7. The minimum value of $\frac{ZW}{S}$ is given by the pointed bar No. 22, and the maximum value by a square with the diagonal to the wind.

The values of $\frac{ZW}{S}$, $\frac{Z}{S}$ and ZW could be plotted in the same way as the values of K, and much interesting information would doubtless be obtained.

In conclusion, the author would acknowledge his indebtedness to Messrs. W. H. Cooper, B.Sc., E. E. Farrant, B.Sc., and F. L. Cunningham, for much painstaking work in making the very numerous observations and the exceedingly numerous calculations connected therewith.

He would most gratefully record his indebtedness to Professor J. T. Morris for much valuable help in making the tests and the calculations relating to the bars shown in Fig. 1. Thanks are due to Commander R. H. Walters, R.N., for valuable help in making the observations. Thanks are also due to the Laboratory Committee of the Aëronautical Society of Great Britain for financial assistance in connection with the expenses. The author would also express his thanks to the Governors of East London College for permission to carry out this work in the Aerodynamical Laboratory at the College.

NOTES

Aeronautics in Japan. Matters of aerial navigation in the Far East, according to the *Japan Times*, are not progressing very rapidly. In 1910 the Japanese Government sent two officers to Europe to study aviation, Captain Hino going to Germany and Captain Tokugawa to France ; and when they returned as pilots they brought with them a number of various types of machines, Blériot, Wright, Grade, and Farman. The Government then established a school for the training of officers "about two hours railway journey from Tokyo," which was afterwards placed in the hands of the Military Aeroplane and Balloon Investigation Society, an official body organised by the Government, its members all being Government employees, including military and naval officers, and professors at the Imperial University. To this Society a grant of £75,000 was recently made, but their activities at the moment are at a pause, since all the foreign machines have been disabled through repeated trials, and the only native aeroplane available is "Tokugawa, No. 1," a purely experimental machine. There are, in addition, quite a number of private constructors, such as Baron Iga, Dr. Narahara, and Messrs. Morita, Kurato, Uyeda, Shimidzu, and Hori, but nothing very successful has so far been produced.

In ballooning the chief work is being done by Mr. Yamada, whose No. 2 non-rigid dirigible has lately done some successful flights. He is at present engaged in constructing a very large dirigible, which is expected to be finished by June. The Military Society, referred to above, has a Parseval on order from Germany.

Aeroplane Fatalities. The already lengthy list of fatal accidents still grows, but it is assuring to be able to add that the percentage in relation to the number of aviators is steadily diminishing. Among the better-known names included in the present list is that of Professor Montgomery, one of the greatest of the American pioneers. His work and research into the subject of gliding and warping is of the highest value, and it is hoped that sufficient data will be found among his papers to compile a complete record

of his labours. No news has come to hand of Mr. Allen, who started from Hendon on April 17 to fly to Dublin, and was last seen flying over Holyhead. It is to be feared that he has met a similar fate to that of Mr. Cecil Grace.

Date.	Name.	Nationality.	Machine.	Place.
1911—				
Sept. 7* ..	C. Tenaud	Peruvian	Blériot (1)	Lima
Oct. 27 ..	Desparmet	French	Blériot (1)	Rheims
Oct. 31 ..	Prof. J. Montgomery	American	Montgomery Glider	San Jose (Cal.)
Nov. 2 ..	Schinreaneck	Austrian		Pilsen
Nov. 16 ..	A. Pietschker	German	Pietschker (1)	Johannistal
Nov. 19† ..	L. Maruzzio	Italian	Blériot (1)	Sebastopol.
Nov. 25 ..	Lieut. F. von Loringhoven	German	Albatros (1)	Doberitz
Nov. 26 ..	U. de Croce	Italian	Chiribiri (1)	Turin
Nov. 28 ..	M. Mosca	Austrian	——	Wiener Neustadt
Dec. 3 ..	A. Reeb	German	M. Farman (2)	Breitenfurt
Dec. 3 ..	T. Shriver	American	Witteman (2)	San Juan, P.R.
Dec. 6 ..	H. Oxley R. Weiss	English German	Blackburn (1)	Filey
Dec. 7‡ ..	Lieut. P. Loder	French	Farman (2)	Versailles
Dec. 13 ..	Lt. C. G. Lantheaume	French	Blériot (1)	Melun
Dec. 29 ..	Cariolon Monic	Montenegran	——	Drowned in L. Scutari
1912—				
Jan. 12 ..	M. Ruchonnet	French	Ruchonnet (1)	Senlis
Jan. 20 ..	A. Wagner	French		Juvisy
Jan. 21 ..	Lieut. Boerner	French	Borel (1)	Senlis
Jan. 22 ..	Rutherford Page	American	Curtiss (2)	Los Angeles
Feb. 3 ..	F. Fraschi	Italian	Chiribiri (1)	Mirafiore (Turin)
Feb. 5 ..	Capt. O. le Magnet	French	——	Saint Cyr
Feb. 8 ..	Capt. Ianich Capt. Leon	Russian	Farman (2)	Johannistal
Feb. 15 ..	A. Schmidt	German	Farman-type (2)	Johannistal
Feb. 17 ..	D. Graham Gilmour	English	Martin-Handasyde (1)	Richmond
Feb. 23 ..	Lieut. Ducourneau	French	Blériot (1)	Pau
Mar. 10 ..	Mlle. S. Bernard	French	Farman (2)	Etampes
Mar. 13 ..	Lieut. H. P. Sevelle	French	Blériot (1)	Pau
Mar. 15 ..	Gustav Witte	German	Wright (2)	Teltow
Mar. 15 ..	Sub.-Lieut. Bertoletti	Italian	——	Vizzola (Ticino)
Mar. 23 ..	Sub.-Lieut. Albokinoff and assistant	Russian	Farman (2)	Sevastopol
Mar. 26 ..	Kleine	German	—— (2)	München-Gladbach
Mar. 30‖ ..	Capt. R. G. Jost	French	—— (1)	Etampes
Apr. 1¶ ..	Lieut. Roberti	Italian	Nieuport (1)	Benghazi
Apr. 5 ..	G. Rodgers	American	Wright (2)	Los Angeles
Apr. 13 ..	Lieut. Boncour	French	——	Laimont
Apr. 17 ..	J. Verrept	French	Borel (1)	Chateaufort
Apr. 18 ..	D. L. Allen	English	Blériot (1)	Irish Sea
Apr. 19 ..	Lieut. de Villed' Avray	French	Blériot (1)	Verdun
May 1 ..	Hösli	Swiss	Gödecker (1)	Johannistal
May 5 ..	Comte de Robillard Cosnac	French	Antoinette (1)	Grimaude nr. Antibes
May 7 ..	Pachmayer	Bavarian	Häfelin (1)	Johannistal

* Date of death. Accident in February, 1911. Omitted from list in October 1911 issue.

‡ Date of accident, died a few days later.

† Accident occurred on May 6, 1911.

‖ Date of death. Accident on February 28.

¶ Reported killed by Turkish artillery. Unconfirmed.

(1) Monoplane (2) Biplane.

NEW BOOKS AND PUBLICATIONS

THE MECHANICS OF THE AEROPLANE. Capt. Duchêne (du Génie). Translated from the French by J. H. Ledeboer and T. O'B. Hubbard. London: Longmans, Green. 1912. Illus. pp. 400. 7s. 6d. n.

*RECONNAISSANCES EN AÉROPLANE. Capt. Pichot-Duclos. Paris: Chapelot, 1912. Illus. and Maps. pp. 132. 3 francs.

*DER FALLSCHIRM SEINE GESCHICHTLICHE ENTWICKLUNG UND SEIN TECHNISCHES PROBLEM. Gustav von Falkenberg. Berlin: R. C. Schmidt. 1912. Illus. pp. 189. 6 marks.

THEORIE UND BERECHNUNG VON MOTOR LUFTSCHIFFEN. Dipl.-Ing. C. Everhardt. Berlin: M. Krayn, 1912. Illus. pp. 204. 10 marks.

JAHRBUCH DER LUFTFAHRT, 1912. Edited by Ansbert Vorreiter. München: J. L. Lehmanns. 1912. Illus. pp. 686.

REPORT ON THE TESTS OF PETROL MOTORS IN THE ALEXANDER MOTOR PRIZE COMPETITION, 1911. London: H.M. Stationery Office. 1912. Plates. pp. 19. 9d.

AERIAL ENGINEERING. Prof. A. Laurence Rotch. Reprinted from *Science*, N.S. Jan. 12, 1912. pp. 11.

*NOTE SUR LE VOL DES OISEAUX. E. Delsol. Paris: Gauthier-Villars, 1911. Illus. pp. 22. 1 franc.

* Reviewed in this issue.

REVIEWS

Stability and Aviation. By Prof. G. H. Bryan, Sc.D., F.R.S. (London: Macmillan. 1911. pp. 192. 5s. nett.)

Introductory Remarks. This is a very interesting book, well worth the attention of all designers of heavier than air flying machines. "Stability" is undoubtedly one of the most pressing aeronautical questions of the day, and a strict, careful mathematical investigation of the principles upon which it depends, is, as for every other mechanical problem, a matter of the highest importance. Briefly the results of the author's investigations may be summed up as follows:—

1st. Mathematical analysis can be successfully employed in cases of this description, to ascertain the general principles upon which "stability" depends;

2nd. The result of the analysis shows that there should be no difficulty in securing inherent stability both longitudinal and lateral by means of suitably-placed auxiliary surfaces rigidly attached to the machine, provided that account is taken of the effects of the inclination of the flight path, &c.;

3rd. Automatic stability, viz., stability obtained by gyrostats, pendulums, &c., is not only unnecessary, but undesirable, as movable parts of this description increase the number of degrees of freedom of the machine, and add to the already large number of conditions which have to be satisfied to ensure stability.

General idea of the Method of Investigation. In order to get a basis for the analysis, an elementary case is taken. The machine is supposed to be moving in still air, and making free oscillations due to the action of a disturbing force, which has ceased to exist. The investigation itself really consists of two parts, viz.: the mathematical theory and the experimental work necessary to provide the details to be inserted in the equations deduced from the theory. As regards the mathematical theory, this is of course the same as that for any other moving body, but the author's chief aim has been to adopt a fairly simple system, and one which will serve as a foundation for more detailed investigations. In this aim he has succeeded, but the subject is a complicated one, and a good knowledge of the mathematical theory of moving bodies is required by the reader.

The experimental work is only generally discussed by the author. He assumes the "Sine" and "Joessel's" laws to be approximately correct for the cases considered, and although the results obtained are not of course absolutely accurate, they are sufficiently so to give a good general idea of the principles upon which the stability of these machines are based.

The General Mathematical Theory. The details of the theory are given in Chapter III. where the case of a machine making free oscillations in still air is investigated in order to illustrate the general principles involved. The centre of mass of the aeroplane is taken as the origin, and three axes at right angles to one another and moving with it in space, are used for forming the equations of motion. A special scheme for the angular co-ordinates is given, as the author considers this particular arrangement to be the most suitable one for the study of small oscillations. Six general equations of motion are formed which ultimately separate into two groups of three each, the first representing oscillations in the vertical plane only, called "longitudinal or symmetric oscillations," the second, containing rotational motions about the x and y axes, and motions perpendicular to the vertical plane, called "lateral or transverse, or asymmetric oscillations." The most important results obtained are, that in the case under consideration the longitudinal oscillations are independent of the "asymmetric" ones, but that, as the latter comprise both directional rotation and lateral motion, "lateral and directional stabilities" cannot be separated.

The general principles of "longitudinal stability" are next considered and Routh's biquadratic equation formed; it being shown that for stability, the roots if real must be negative, or if imaginary must have their real parts negative. [For details see "Stability of Motion," by E. J. Routh.] "Asymmetric" stability is then discussed, and the conditions for stability found in very much the same way. The detailed investigation of the character of the oscillations in both cases is considered later.

The Experimental Work. As explained above, the details of this are only briefly discussed, but a few notes on the author's method of applying the results of the experiment may be given. He calls the data obtained "Resistance Derivatives," and divides them into two classes, viz.: "translational derivatives" due to the pressures caused by the translational movement of a body or surface through the air, and "rotary derivatives" due to motion such as pitching, rolling and yawing (orientation). The derivatives of the former class are fairly well known to the aeronautical engineer, as a good many experiments have from time to time been made on the air pressures, &c., acting on surfaces and bodies of different kinds. The "rotary derivatives" on the contrary have hardly been studied at all, and the author makes some suggestions for experimental work in connection with them. As regard this part of the subject the views of Hovgaard [see "The Motion of a Submarine, &c." Proceedings Institution of Naval Architects] are interesting. In his preliminary investigation Commander Hovgaard omits these "angular resistances," as he considers they complicate the solution too much, but in his final analysis he examines them, and states that, owing to them, the angular motion is extinguished sooner than would otherwise be the case, but that the general character of the motion is not influenced. The author's views appear to be that, when narrow planes are used and stability is secured by auxiliary surfaces, such as a tail, the "rotary derivatives" are negligible; but there is no doubt that further experiments on this class of derivative is much required. If it is not necessary to consider them, as seems possible when flexible surfaces like the wings of a bird come into use, the whole question of stability will be very much simplified.

Longitudinal Stability of Single Lifting Systems. In Chapter V. the case of single lifting machines, viz., those in which no weight is carried by the tail, is considered. The simplest case, viz., a supporting plane, with a tail in rear, and having the line of action of the propeller thrust passing through the centre of mass, is taken, the value of the "Resistance Derivatives" calculated, and the biquadratic equation of stability found as previously described. The result of the investigation is important, as it is shown that the oscillations must be divided into two classes, viz., short and long, the latter being the most important, as the stability may be very much affected by them. They only however exist, as the equations show, under certain circumstances, viz., when the machine is disturbed by forces acting, say, horizontally, directly ahead or astern, as in such cases certain terms (μ or sin α) cannot be neglected as in the case of the short oscillations, and consequently different values are obtained for the roots of the stability equation. Another very important matter discussed in this chapter (Mr. E. H. Harper) is the effect of the inclination of the flight path on the stability, and it is shown that, under the given conditions, a machine becomes unstable, if the tangent of the angle of elevation of the flight

path has some value, less than twice, that of the angle of attack. The effect of "head resistance" (viz.: the resistances of the body of the machine, motor, aeronaut, skin friction, &c.), is next explained, and it is shown, as would naturally be expected, that head resistance increases the longitudinal stability. In connection with this branch of the subject, it is desirable to point out that the body of a machine (which provides most of the head resistance) should be designed on scientific principles and not left to chance. In the case of birds, the body seems to bear a definite relation to the particular kind of wing used, being designed, not merely as a form of least resistance, but also with a view to the effect it will have on the bird's stability. This is a most important matter, and one which should be very carefully considered by the constructors of full-sized flying machines.

Longitudinal Stability of Double Lifting Systems. In Chapter VI. the case of machines in which the weight is distributed over two or more narrow planes placed tandem is discussed. It is shown that for every "double lifting system," there is an equivalent "single lifting system," having the same conditions of equilibrium and stability (and the same total superficial area of the planes) and that as a consequence of this, the calculations connected with the stability can be much simplified.

The question of broad planes is next examined, as also the effects of friction, deviation from the "Sine" law, &c. It is perhaps here desirable to remind the reader that while the "Sine" and "Joessel" laws are useful for showing the general principles of stability, they must not be assumed when calculating the details for an actual machine. The real laws differ from them very considerably, and a design based upon them would almost certainly be very seriously in error.

Asymmetric Stability of Straight Plane Systems with Fins. In Chapter VIII. this question is discussed, the machines under consideration being assumed to have planes perpendicular to the vertical plane. It is pointed out that the conclusions arrived at, as to long and short oscillations in the case of longitudinal stability, do not apply, and that the separation of the roots to find the character of the oscillations must be done in a different way; a general explanation of the system employed being given. An important question is dealt with on page 139, viz.: the possibility of loss of lateral stability when a machine dives downwards at an angle greater than that consistent with the relation

$$\tan \theta < 2 \tan \alpha \text{ (see above).}$$

In a case of this kind a serious accident might occur, and as the author shows, the real reason for it would probably escape notice. Many other interesting matters are discussed in this chapter, such as effect of head resistance, friction, curvature of main planes, &c.

Asymmetric Stability of Bent-up Planes. This is discussed in Chapter VIII., and a detailed investigation of the "Antoinette" type of machine (Mr. E. H. Harper) is given. This latter is very interesting, as it shows that in this case lateral stability can be got in two ways. If the tail is neglected it is sufficient to raise the dihedral angle of the wings above the centre of gravity (within certain limits), or if the wings are not raised, the tail must be made not less than a certain length. Hence, by a judicious combination of raised wings and correctly designed tail, the stability may be made independent of the flight path.

Miscellaneous Problems. In Chapter XI. a list of miscellaneous problems connected with flight is given, such as:—

(4) The determinations of the initial motions set up when an aeroplane is suddenly struck by a gust of wind in any direction, longitudinal, lateral, or vertical, or compounded of all three;

(5) Stresses set up in aeroplanes;

(9) Experimental study of effects of inclination of flight path; &c., &c.

Many of these are of great importance, and it is to be hoped that steps will soon be taken to solve them.

Graphic Statics of Longitudinal Equilibrium. A detailed account of this subject is given in Chapter IV.; it will be found useful by those who prefer graphic methods.

Concluding Remarks. It will be seen from the above that the book is a very interesting one, and worthy of serious study by designers of aeroplanes. The main principles of stability are well explained, and it is clearly shown that under the assumed conditions

" inherent stability " both longitudinal and asymmetric is perfectly feasible. As regards, however, the application of the theory to actual machines, the author is perhaps rather optimistic. The real difficulty in problems of this kind is not so much the mathematical theory as the collection of the necessary data to insert in the equations. For the nonce a certain amount of information regarding the translational motion of bodies, &c., advancing along a horizontal path, is available, but of lateral movements or of the effect of the rotary derivatives very little is known. No doubt this state of affairs will be improved upon later, but the many difficulties in connection with this subject should be borne in mind by present-day designers.

The diagrams are as a rule good, but Fig. 2 seems to require revision, as the lettering does not apparently agree with the text. A word of praise must be given to Mr. E. H. Harper ; the work done by him is very interesting and valuable.

J. D. FULLERTON,
Col. R.E. (ret.)

Reconnaissances en Aéroplane. By Capt. Pichot-Duclos. (Paris : Chapelot, 1912. Illus. and maps. pp. 132. 3 francs.)

THIS extremely interesting and suggestive work goes far on the way to become a standard text-book. The question of fighting in the air is not touched, as it is really outside the scope of the book, and the author devotes his attention to two main points : the suitable machine and the training of observers. Various types of machines are examined to find one that gives the maximum range of view to the observer, for this quality is laid down to be of the first importance. The conclusions drawn are (1) that biplanes are superior to monoplanes for detailed reconnaissance work ; (2) that " biplaces " or two-seated aeroplanes are the most useful ; (3) that " monoplaces " or single-seated aeroplanes are necessary for simple work where speed is the predominant factor.

The only monoplane type that seems to come well up to the conditions required is the Paulhan-Tatin. Monoplanes of the Train and Bleriot 11 bis type are commended, but have the fault of obstructing the overhead view and shutting out sight of possibly hostile machines. The ordinary monoplane type (including Etrich, Nieuport, Déperdussin) are blinded by the wings, the motor and the propeller ; and " windows " in the wings and the floor of the cockpit are but a poor mitigation of the evil.

In biplanes, the Zodiac, the Voisin Canard, and the Sommer, Maurice and H. Farman are commended as giving a good view to the observer ; and on general points such as safety and ease in landing, wind-screens and general adaptability, points are specially awarded to the M. Farman. Apart from the question of land transport, this aeroplane seems to be an exceedingly suitable one for military work.

Turning to observers, Capt. Duclos urges a better recognition of their services and fuller grasp of the high training necessary for obtaining results of value Taking into account the human factor, there must be good accord between pilot and observer, and the observer must have got over the idea that he is not so much " in the lime-light " as the pilot and must not be disdainful of his work. In the relations between the two, it seems necessary that the observer should be in command, and therefore, where possible, of higher rank than the pilot. Capt. Duclos gives some striking instances of the uselessness of untrained observers, and drives home the fact that not only should the air-scout be able to tell at a glance the number and nature of the forces observed, but also their tactical position and objective.

The book towards the end gets somewhat scrappy and disconnected, and would be the better for re-arranging and editing, but it can be confidently commended to the notice of all those interested in this subject, particularly to members of the Royal Flying Corps.

T. O'B. H.

Der Fallschirm seine geschichtliche Entwicklung und sein technisches Problem. By Gustav von Falkenburg. (Berlin : R. C. Schmidt, 1912. Illus. pp. 189. 6 marks.)

THE parachute has at last found an exponent, and in this unique volume, its history, theory, mathematics, and scientific application are skilfully and cleverly set forth. The history of the parachute is long and fascinating, beginning with Leonardo da Vinci, who undoubtedly invented it, and is brought up to the present day. The familiar umbrella shape has survived and predominated, not because of its scientific construction—as a matter of fact its form would seem particularly unscientific—but chiefly because it has

been neglected and passed over in the field of aeronautical research. The other forms of parachutes, such as the Cocking or inverted cone, the Köppen or rotating plane, and the broad-bladed screw-shape revolving on a vertical axis—whose originator is unknown, but apparently English—all await the attention of the scientist. Over 100 years ago, Lalande, the astronomer, improved the umbrella-shape by putting a hole in the middle for the purpose of steadying it, and, except for this, it remains to-day substantially as first invented.

Herr von Falkenburg has rendered valuable service to the science in producing this book, which we hope will find a translator in this country, as literature on the subject is almost entirely lacking and only to be found in ephemeral articles.

Note sur le vol des Oiseaux. By E. Delsol. (Paris: Gauthier-Villars, 1911. Illus. pp. 22. 1 franc.)

"THE present note aims at giving a complete explanation of the 'vol plané' . . ."

Claims similar to the above, with which M. Delsol opens his little book, are often advanced on behalf of quite inadequate aeronautical theses. No surprise need therefore be felt when it is found that M. Delsol gives no indication of knowing what has already been done by other investigators of bird flight, although he writes as one who might produce results of value if he were first adequately equipped with the necessary special knowledge of his subject. As it is, the theory which he propounds is of very limited application to the vexed problem of soaring. This feat is imagined as taking place in still air, and to be accomplished by the bird varying the angle of attack of its wings by spiral or sinuous gliding. M. Delsol cannot complain if his hypothesis is ignored until he brings forward something more tangible than at present in support of it.

CORRESPONDENCE

To the Editor of the AËRONAUTICAL JOURNAL.

LAWS OF AERODYNAMICS AND LANGLEY'S LAW

SIR,—May I be allowed to comment on Mr. Turnbull's very interesting article in the AËRONAUTICAL JOURNAL of January ? My desire is to point out in what way Mr. Turnbull's inferences from Langley's and M. Eiffel's experiments may be extended so as to cover more ground, and be brought into collaboration, in a manner of speaking, with the results of investigations which have proceeded on other lines. In doing this, I hope to add to the usefulness of Mr. Turnbull's work, since it is obvious that the more cases can be included under a certain law, the more useful does the law become in design as well as in practice. Regarding Law A, it is, *mutatis mutandis*, nearly Rankine's old doctrine of screw propellers generalized. Law B stands on a somewhat different footing, as it forms a complement to Langley's law, or paradox, whichever it is to be called. In quoting authorities hereinafter I do so from memory, without regard to priority or exhaustiveness, for I am at present away from books, so cannot verify my references and I claim no originality.

With regard to Law B, which concerns maximum load for carrying power per horsepower, Lanchester, for instance, discusses Langley's law, and, after pointing out that it can only be true of perfectly thin—I fear thicknessless is not an English word—and frictionless planes, he finds that, taking account of direct head resistance due to surface friction and framing, &c., there are two notable speeds of any given machine, with given area and load—one, the higher, at which the total resistance is a minimum ; the other, at which the horsepower to carry the load is a minimum. The latter is the same thing as Mr. Turnbull's speed for maximum load per horsepower. The mathematics are simple enough on the assumptions, firstly, that head and frictional resistances vary as the square of the speed, and secondly, that their sources may be represented for purposes of calculation, by a plane of fixed area, square to the wind, the supporting or main plane being treated as frictionless and thin. Neither of these assumptions is in exact agreement with facts, no doubt, but it will hereinafter appear that for most purposes, the agreement is sufficiently near to be useful. There is no difficulty, and I need not trouble you with the formal proof, in showing that, on the above assumptions, if we put load carried per horsepower = l ; square feet of wing area per 1000 lbs. or other unit load = s ; speed = V, and if V be the speed for which load carried per horsepower is a maximum, then in Figs. 2, 3, 4, 5, 6, and 7 of Mr. Turnbull's article, the curves in Figs. 2, 4, and 6 are common hyperbola, in which $lV = L$, a constant ; and in Figs. 3, 5, and 7, the curve which starts from the origin is a common parabola, $l^2 = Ms$ where M is a constant ; and the other curve is a kind of hyperbola in which $sV^2 = N$, a constant also. The constants will, of course, differ in the three sets of experiments, owing to the planes experimented on differing from one another in various ways, and will, like most engineers' "constants," vary a bit from point to point along the curves. On testing the figures arithmetically, the constants turn out to be quite good constants, considering the generally notorious bad character of hydrodynamical constants, particularly those connected with the propulsion, &c., of ships, to which these are nearly akin.

The numerical values of the "constants" taken from the curves as printed in Mr. Turnbull's article are as follows—minute accuracy being neglected :—

Fig.	$lV = L$	Fig.	$l^2 = Ms$	$sV^2 = N$
2	$78 \times 32 = 2500$ $60 \times 43 = 2600$ $33 \times 73 = 2410$	3	$33^2 = 5{\cdot}44 \times 200$ $48^2 = 5{\cdot}76 \times 400$ $74^2 = 6{\cdot}08 \times 900$	$216 \times 70^2 = 1060$ $425 \times 50^2 = 1060$ $900 \times 35^2 = 1040$ } $\times 1000$
4	$77 \times 26 = 2000$ $63 \times 32 = 2000$ $57 \times 37 = 2100$	5	$37^2 = 6{\cdot}82 \times 200$ $52^2 = 6{\cdot}76 \times 400$ $70^2 = 6{\cdot}13 \times 800$	$200 \times 57^2 = 650$ $400 \times 40^2 = 640$ $1000 \times 25^2 = 625$ } $\times 1000$
6	$228 \times 22 = 5020$ $163 \times 31 = 5050$ $125 \times 42 = 5250$ $93 \times 56 = 5200$	7	$97^2 = 47 \times 200$ $148^2 = 55 \times 400$ $180^2 = 54 \times 600$ $220^2 = 54 \times 900$	$200 \times 53^2 = 560$ $500 \times 31^2 = 480$ $800 \times 24^2 = 460$ $1000 \times 21^2 = 440$ } $\times 1000$

These figures show that the reason the experiments he cites give the results as to speed, area, and maximum load per horsepower which Mr. Turnbull extracts from them, is just very nearly that which has formed the basis for the approximate equations of the curves. In other words, Law B is the consequence of the existence of direct resistance, nearly resembling that of a plane of constant area, facing the wind, attached to a supporting plane having no thickness or friction, which would, by itself, follow Langley's law. Langley himself assigned as a reason for the curve A of his experiments (see Mr. Turnbull's Fig. 1) being what it is, the thickness and square edge of the experimental plane, which at small angles of attack constituted a resistance plane of sensible area, and he corrected the curve A accordingly. We need not then hesitate to extend the application of the explanation of Law B above given, to other cases, and in particular to the case of minimum resistance.

The speed of minimum horsepower for a given load, otherwise the speed of maximum load for that horsepower, would, I believe, be found to be in practice very inconvenient and perhaps dangerous, because any casual disturbance of that speed, whether it be increase or decrease, calls for increase of horsepower to correct it. The speed of minimum resistance has the advantage that casual disturbance of the speed, if small, scarcely alters the resistance at all, and if considerable, it reduces the horsepower necessary for support if it reduces the speed, because the speed of minimum horsepower is less than that of minimum resistance, and if it increase the speed, the engine thrust usually diminishes, so that the disturbance tends, in a manner, to correct itself ; and there is a good margin for loss of power from accidental causes, such as missed ignitions, &c., to fall back on before the minimum horsepower limit is reached. I suppose, indeed, that for very sufficient reasons regarding safety and controllability, aviators would, intentionally or unintentionally, arrive at flying small or medium power machines at speeds lying between that of minimum horsepower for their lower, and of minimum resistance for their upper, limit. The range of speed is about 30 per cent. of its lower limit, and the range of power about 15 per cent. of the same.

High power machines may, of course, be made to fly at speeds exceeding that of minimum resistance, but if they do, it is at the sacrifice of total distance or range of action with a given supply of fuel, and a sacrifice of manageability in landing, arising not only from their minimum power speed being not low enough for convenience, but from their being likely to suffer loss of stability just before landing by the increase of angle of attack, necessary with the reduced speed, involving the pointing upwards of the machine as a whole, a source of instability pointed out by Prof. Bryan as an effect of upward inclination of the propeller thrust to the line of motion, though it favours stability if the inclination be downward.

The speeds of minimum resistance and of maximum load per horsepower, otherwise that of minimum horsepower for a fixed load, both greatly depend, as Mr. Turnbull's figures show, on one particular thing, namely, the ratio of head resistance to drift of supporting planes, because it is that dependence which enables the forms of the curves in his figures to be predicted. It is probable, I think, that this ratio is a sufficiently small percentage to make it feasible to provide means, either by the ailerons or otherwise, for making, a little before landing, a large increase in the head resistance of high speed machines and of the angle of attack, without requiring the machine to be lifted upwards, thus enabling the speed to be reduced within convenient limits. I daresay the thing is actually done in many machines, but I think it very likely that the reason is not well understood, and the thing would be better done, both as regards design and execution, if done wittingly with set purpose and understanding.

Also, the speed of minimum horsepower could, I fancy, be found for any particular machine, by a skilful pilot on a calm day. The ratio of the economical speed to it is an invariable constant, not depending on design, &c., consequently the latter (the useful speed) would be ascertained. The outcome seems to be, as regards both Langley's and the other experiments, that it is impossible to prevent edge eddies and surface fric ion influencing the experimental values of the coefficient κ in the formula: Pressure $= \kappa A V^2 \sin \beta$ where A is the area, V the speed, and β the angle of attack, and in other similar formulæ quite appreciably at small angles of attack. The correction for these disturbing influences can, however, be made with apparently sufficient accuracy by finding the point of minimum horsepower, and possibly the coefficient of surface friction can be separated from that belonging to edge eddies. Langley's plate was thick enough, if I remember rightly, for its square edge—it was not sharpened—to have, as he remarked, a quite appreciable effect ; but other experimenters have worked, I think, with much finer edges. I do not know what sort of edges M. Eiffel's planes had.

MAURICE F. FITZGERALD.

P.S.—I think it is unfortunate that Mr. Turnbull did not happen to test the relation between minimum horsepower per 1000 lbs. and speeds corresponding, it being approximately linear, and this circumstance could not have escaped attention.

SIR,—I am very glad indeed that Mr. FitzGerald has taken up the discussion of the laws stated in my article in the January issue of the JOURNAL, for I quite agree with him that the more cases that can be included, under a certain law, the more useful does the law become.

The thorough " thrashing out " of " Law A," as applied to all devices for obtaining dynamic support from the atmosphere, and of " Law B " as applied to the inclined plane or aeroplane, is, at the present time, of great importance to the science of aerodynamics. If the laws are true and absolutely *general*, as I feel assured they are, they furnish a corner-stone on which to build both theory and practice. When we remember that there were locomotives 25 years before Stephenson's " Rocket " which was the prototype of all modern locomotives), we should not feel *certain* that we have, in the present-day aeroplane, the prototype of the dynamic flier of the future, that shall encompass aerial navigation, in the broadest sense, *in any weather*—the aeroplane we have, and it is a great step in advance; time alone, however, can decide whether it is the prototype of the future machine or not. In any case, it is of the greatest importance that the general laws of aerodynamics be formulated and understood as early as possible; theory, laboratory experiments, and field practice should go " hand-in-hand " for rapid and thorough progress.

To return to Mr. FitzGerald's letter, I do not think that " Law B " can be properly called " the complement of Langley's law "—" Law B," as I understood the matter, is the *complete general* law (in regard to efficiency) of the inclined plane and aeroplane, and *includes* " Langley's law " (as heretofore known) as its first phase. This first phase might have an infinite ordinate (I refer to Figs. 2, 4, and 6), if it were possible to conceive of a plane *without thickness and without skin-friction;* but if it has any thickness there is drift, and if it has any surface there is also drift due to skin-friction. We may imagine a plane infinitely thin, but we cannot imagine a plane without surface, *as it would cease, by definition, to be a plane;* it seems apparent, therefore, that *even as a wholly theoretical case,* " Langley's law " can only obtain as the first phase of " Law B," and this first phase can attain a maximum *finite* ordinate only.

Mr. FitzGerald draws attention to Lanchester's statement that " Langley's law " only holds true in the case of a plane infinitely thin, but it cannot hold true even for a plane infinitely thin, for *we cannot have a plane without surface*, as pointed out above. I think we are, therefore, justified in believing that " Law B," in its general form of expression, applies as well to the mathematical plane of no thickness, to the material plane with no supporting members (*i.e.*, members that can be corrected for), and to the complete aeroplane with supporting members and with body and strut resistances of various kinds.

Mr. FitzGerald's deductions as to the mathematics of the curves are interesting, but I think we should be a little more exact in our statement concerning the formulæ. The curves in Figs. 2, 4, and 6 are obviously not common hyperbolæ *themselves*, but the curve joining the maximum ordinates is apparently of hyperbolic form,* as Mr. FitzGerald shows. I do not think we can find suitable equations for the curves themselves (in Figs. 2, 4, and 6) applicable to the various cases; both the first and second phases (in certain cases) remind one of the Cissoid, or possibly the Cycloid, but neither the equations of the Cissoid or Cycloid express even the first phase.

As regards the maximum lift curves of Figs. 3, 5, and 7, the equation of the parabola certainly fulfils the conditions with reasonable accuracy, when we take the curves between the limit of 200 and 1,000 sq. ft. per 1,000 lb.; but when we extend the limits to 100 sq. ft. the constant, M, will vary considerably from the average value.

Regarding the " speed at which maximum occurs " curves, of Figs. 3, 5, and 7, described by Prof. FitzGerald as " a kind of hyperbola." While the equation $SV^2=N$ seems to answer very well for the curves, between the limits of 200 and 1,000 sq. ft. per 1,000 lb., it does not apply so well when we extend the limits to 100 sq. ft., as here again the constant, N, is subject to a considerable variation from the average value.

Before publishing the curves in the January issue of the JOURNAL, I examined them to obtain formulæ that would properly express them, and came to the conclusion that in our present state of knowledge, at least, it is perhaps better to abide by the curves rather than formulæ. To be sure, at the present time, the limits of nearly all practical aeroplanes lie between the limits of 200 to 1,000 sq. ft. per 1,000 lb., but it is difficult to say what the future may bring forth, and, if the curves more faithfully represent the

* The equation $xy=$a constant, is that of a hyperbola referred to its asymptotes as axes.

actual relationships than formulæ, they are to be preferred. Nevertheless, it is certainly important to keep the proposed formulæ in mind, more accurate experimental data may bring the curves and formulæ in closer accord and, if we find that formulæ can be properly applied to our aerodynamic curves, they furnish a very valuable means of generalising our ideas, and would help on the progress of practice as well as theory.

With respect to Langley's reason for adopting curve B, in preference to curve A (Fig. 1 of the original article), Langley was undoubtedly justified in *lowering* curve A (between the angles of zero and twenty degrees) on account of the square-edged planes used,* but he was not justified in producing the curve to zero, for by so doing he ignored the effect of skin-friction, as previously pointed out. This may seem a very small matter to argue about, but it is precisely this small drift at low angles of inclination (which must always exist *with any surface*) that turns " Langley's law " into the first phase of the general " Law B." In actual flight the low angles of incidence occur at the highest speeds, and since the drift, as well as the lift, is affected by the V^2 law, it makes a very material difference whether we accord to drift (at zero degrees) a positive or zero value.

Turning to Mr. FitzGerald's deductions for practical cases, I cannot quite agree with the idea that the speed of minimum horse-power is necessarily either inconvenient or dangerous (except, of course, as the question of speed applies to stability—a question always of importance). I would, however, be the last to advocate no reserve of power, as aeroplanes and motors stand to-day, but it is quite conceivable that an aeroplane be designed for a maximum load per H.P., and that a motor be so designed that it works at best efficiency (at the designed aeroplane efficiency and speed) while holding in reserve considerable additional power applicable, when desired, at the will of the operator, by throttle or other control. Moreover, as Mr. FitzGerald suggests, a normal speed between that of minimum horse-power and minimum resistance, may be found to work out best in practice.

In my article I purposely refrained from dealing with the question of speed of minimum resistance, as I hope to treat this subject in a subsequent paper, and my one mission was to place " Langley's law " in a clearer light, and to substitute the more complete law, which includes " Langley's law " as its first phase. It goes without saying, that the speed of minimum resistance, in contrast with that of minimum power, is a matter of much interest and importance, but there are certain cases where the two speeds lie very close together, and the whole question is one quite worthy of a special discussion.

I am glad that Mr. FitzGerald draws attention to the linear relation between minimum horse-power for a given weight and the corresponding speed, as the matter escaped my attention, but here again we must not jump to a too hasty conclusion. Upon plotting the results we find that the relation is well expressed, in the case of the plane surfaces, by a straight line ; but it is more accurately expressed, in the case of curved surfaces, by a curve ; this is a matter that I propose discussing more fully in the article on the speeds of minimum resistance and horse-power.

I would like to take this opportunity of making some additions to the text of the original article that should make some portions a little clearer.

Errata and Addenda.

P. 45—ll. 19 and 28—after " infinitely thin," add : " and without skin-friction."

P. 46—l. 10—instead of " or miles per hour," read : " and miles per hour."

P. 46—l. 19—after " in the curves of Fig. 2," add : " It is to be particularly noted that in Figs. 3, 5, and 7, ordinates are pounds lift per H.P. for the maximum lift curve, and are *also* miles per hour for the speed curve."

P. 46—l. 6 from bottom—after " sustained," add : " this confirmation is shown graphically in Figs. 3, 5, and 7 by the curve marked speed at which maximum occurs."

P. 46—add to footnote :—" But Eiffel, in the more accurately drawn curves given in his subsequent work, *La Resistance de l'Air et l'Aviation*, Paris, 1910, *does not* produce his drift curve to zero. This would somewhat alter the shape of curves in Figs. 4 and 5, and make them more similar to the refigured Langley results of Figs. 2 and 3."

P. 48—line 10—after " Langley's law," add : " but is true of the Langley-phase as above defined."

W. R. TURNBULL.

Rothesay, N.B., Canada.
March 20th, 1912.

* Eiffel's plane and curved plate also had square edges, the thickness being 3 mm. with plan dimensions of 900 mm. by 150 mm.

AERODYNAMICAL COEFFICIENTS ON CAMBERED SURFACES

SIR,—There seems to be a considerable difficulty in devising a suitable rule to express lift and resistance for aerocurves. In Mr. Page's paper to the Society (February 14th, 1911) he adopts the rules followed by Rateau and Soreau:

$$L \propto (\theta + d)$$
$$R \propto (a\theta^2 + b\theta + c)$$

θ is chord angle of incidence; d is a "deviation" angle.

Mr. Berriman, in his criticism of the said paper and his later contributions to the British Association and Royal Society of Arts, adopts Lanchester's form :—

$$L \propto (\alpha + \beta)$$

where α is the entry angle, β the trail angle, $\alpha + \beta$ the "deflection angle." In my paper on Propellers (AËRONAUTICAL JOURNAL, July, 1911, p. 112) a similar line is followed.

The objection to the first method lies in :—

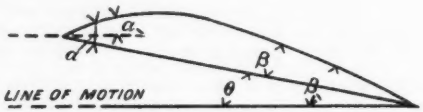
(1) The uncertainty as to d;

(2) The anomalous increase in the "sweep factor";

(3) Irregularities or even negative values of b.

The second method is open to the very palpable difficulty that the total reaction is apparently invariable, the variations in lift being simply due to the change in the direction of the resultant pressure.

May I mention that the following method seems to agree fairly well with the results.



(1) *For perfectly continuous stream line motion—*

$$L \propto (\alpha + \beta) \text{ or } (\alpha_1 + \beta_1)$$

There is only one position in which this can occur and for this position the lift-to-drift ratio is a maximum.

(2) For smaller values of θ there is a *discontinuity* behind the dipping edge. (Incidentally this causes the c.p. to recede.)

(3) For larger values of θ there is a *discontinuity* at the *upper rear surface.* (This again causes the c.p. to recede so that the c.p. is most advanced in the position of perfect flow.)

Numerous experiments have shown that when θ is between 3° and 8° (mean, say, 5° or 6°), L/R is a maximum, so that for the position of maximum L/R :—

$$\alpha_1 = \alpha - 5°$$
$$\beta_1 = \beta + 5°$$

Experiment also indicates when $\theta = -5°$ or thereabouts, L/R = o.

On this basis a formula,

$$L \propto [\alpha_1 + \beta + \theta]$$

would seem to give satisfactory results, θ being the only variable, α_1 being the *entry angle in the position of stream line flow* (maximum efficiency) and β the trail-to-chord angle.

The resistance is a more difficult item to express. The resultant is inclined forward of the normal to the trail tangent plane and for the best value of θ, is apparently almost exactly perpendicular to the chord, so that

$$R = P \sin \theta$$

or approximately L sin θ.

As θ decreases R does so slightly, being a minimum when $\theta = o$, and is then almost wholly skin friction.

In the paper of mine referred to above, L/R is computed somewhat on these lines.

As to the effect of aspect ratio, I am inclined to regard this as purely due to the relative elimination of end-contractions of stream which must occur as the aspect ratio increases. Employing Lanchester's rule,

$$L \propto Cc,$$

I find that for greater aspect ratio than 2, the rule

$$c = \frac{3n - 2}{n} \quad \text{(n being aspect ratio)}$$

is fairly accurate.

HERBERT CHATLEY.

Tang Shan, China.
February 19th, 1912.

WIND TUNNEL RESEARCH

SIR,—It has been suggested that a short note on the difficulties to be met with in Wind Channel work might find a fitting place among the correspondence on Mr. Handley Page's paper.

Let it be said at once that it is too soon—probably years too soon—to expect the various Laboratories to arrive at agreement between themselves to a high degree of accuracy, on account of the considerable possibility of error which attaches to Wind Channel work on models. Error may arise from :—

(1) Errors in model making ;
(2) The use of a law of similarity which is incorrect ;
(3) Ordinary errors of observation ; and
(4) Various systematic errors.

Of these, (1) is not usually serious, at least if care is taken, in the case of wooden models, that they have not warped since being made.

As for (2), this, most fortunately, does not make any enormous difference in the case of aeroplane work, but in the case of large work, such as dirigible balloon envelopes, in predicting full scale results from model tests, differences of as much as 30 per cent. may occur between the results obtained by the use of the theory employed by, for instance, Captain Crocco, and that employed by the N.P.L. In view of such discrepancies as these it is to be hoped that some better agreement between the various authorities may soon be arrived at.

Errors of the third class are all those that can be summed up in the phrase " failure to repeat readings exactly." They occur in all classes of experimental work, they are amenable to treatment by the Theory of Probable Errors as given in the text-books, and hence they can always be reduced as far as may be nccessary by a sufficient number of repetitions of the readings. These errors are apt to be rather large in Wind Channel work, owing to the irregularity of the artificial wind, although, be it understood, a natural

wind is far worse. The following table will give an idea of the number of repetitions necessary to get reasonable accuracy, exclusive of causes of error mentioned under the other three clauses:—

Channel.	Probable error in a single observation.	Number of observations necessary to reduce the probable error of the mean to 1%.
Tests on a five-foot square board on a tower in the natural wind at the N.P.L.	10%	100
Four-foot channel at N.P.L. (centrifugal fan, air circulating in a closed casing)	4%	15
Two-foot channel at N.P.L. (propeller fan, free discharge ..	less than 1%	—
Four-foot (1.2 metre) channel at Koutchino (propeller fan, free discharge)	5%	25

Of these, the last case is not fair as a comparison of the performance of the channel in question : the low accuracy being partly due to the smallness of the models in use in the experiments from which the figures are taken.

The fourth source of error, which is by far the worst, accounts for most of the discrepancies between various observers using different methods, and includes among others such items as the following :—

(A) The disturbance of the flow due to the use of a disproportionately large model. This source of error is worse in a wall-less channel than in one of the ordinary type.

(B) Measuring the velocity with instruments which have been incorrectly calibrated.

(C) Using a wrong systematic correction to relate the wind speed at the obstacle to that at the gauge ; this applies particularly to natural wind work.

(D) Error in angular position of a model which is so mounted on a light spindle that the wind forces on it can spring the spindle.

Such then is the present state of affairs ; it will gradually improve, of course ; agreement on points of theory must soon be reached and apparatus proved to involve serious systematic errors will be gradually weeded out, but meanwhile constructors will have to be content with approximations not always of the closest in the matter of absolute values, though as far as concerns relative values any one set of experiments from the same source can of course be trusted rather more fully.

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